UNIVERSITY OF PENNSYLVANIA

ON INSTINCT AND INTELLIGENCE:

A STUDY IN DEFINING FOR EXPERIMENTAL USE

EVA MAY WYMAN

A THESIS

IN PHILOSOPHY

PRESENTED TO THE FACULTY OF THE GRADUATE SCHOOL IN
PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR
THE DEGREE OF DOCTOR OF PHILOSOPHY

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TO

EDGAR ARTHUR SINGER, JR.

and

EDWIN BURKET TWITMYER

My esteemed teachers and friends

PREFACE

The present study was undertaken at the suggestion of Professor E. A. Singer, Jr., as a test of the adequacy of a certain philosophic method to the formulating of scientific questions for experimental answer. The general principle defining this "experimentalist" philosophy has sometimes been put in the brief formula: "No 'question of fact' is known to have meaning, until it is shown to have at least one answer lying within the limits of a measured experimental error."[1]

The question, "How much more intelligent is A than B?", is such a question of fact. Experiments purporting to answer this question fill the psychological literature of the day. But a question cannot be well answered until it is well asked; the literature of the "intelligence-question" reveals chaos of opinion among experimenters as to what is the *right* answer, or even, as to what is the right way of approaching an experimental answer. One may assume the "categories" in which the question has so far been asked to have lacked sufficient definition in the mind of each experimenter to let them (or any question asked in terms of them) have a common meaning for all.

The writer has then begun by reviewing the history of "intelligence" and its historical correlatives (*e.g.* "instinct"), with a view to analyzing-out and reducing to the fewest possible classes the *philosophic motives* underlying the long conflict of opinion. (Chap. I, II). These motives are then exhibited as persisting in current discussion; an effort is made to show their conflict to have resulted from an unnecessary inference drawn by each opponent from a postulate otherwise acceptable to all. A preliminary definition of "intelligence-correlated-with-instinct" is proposed, which, rejecting each historic inference, accommodates all historic postulates (Chap. III).

On the basis of this preliminary definition, the measure of intelligence becomes a problem of *experimental order*. To reduce it to a problem of a laboratory dimensions, a close analogue of the "preliminary definition" is framed in mathematical form. This definition expresses "intelligence" as a certain mathematical

function of "efficiency" and "experience"; of which variables *efficiency* is first made measurable; then *intelligence* and *experience* so "interdependently defined" in successive postulates as in the end to find simultaneous expression in values calculated from laboratory data. (Chaps. IV, V).

The study turns to a critique of the formula used in these calculations. Certain discordant relations having appeared among the results; the author is unwilling to assume that these must tend to disappear with the reduction of experimental error. Chap. VI suggests a first generalization of the original formula. This, in attempting to express *efficiency* as a function of *experience*, with *intelligence* as a sole parameter, may have erred on the side of simplicity. The most general expression relating *efficiency* to *experience* is finally developed; in this, *intelligence* and *instinct* remain the most significant, but not the only parameters entering into the function. In conclusion the way is prepared for an experimental test of the importance to an efficiency growing out of experience, of "faculties" other than intelligence and instinct as these faculties have here been defined.

Permit me here to express my grateful appreciation especially to Professor Singer, philosopher, at whose suggestion this study was undertaken, and under whose guidance it was completed; to Professor Kline, mathematician; to Professor McClung, biologist; and to all members of the Psychology Department of the University of Pennsylvania who suggested helpful information. To the officials of the University Library who provided books, to the two persons who took the preliminary test, and to the five individuals who gave their time and effort to supply laboratory data from which the values in the tables were calculated, I beg to tender my grateful acknowledgments.

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I

The Problem in its Historic Development

Early theories. Although clear suggestion of a dualistic conception of nature is to be found in the teachings of Empedocles, Anaxagoras is usually taken to be the founder of dualism as a system of natural philosophy. Anaxagoras is first to ascribe the motive force in nature to a world-ordering intelligence (*nous*); he is first to distinguish throughout all nature non-physical from physical forces, and to separate the two into categories irreducible the one to the other; he is the first to make explicit the assumption of "mind" as the "cause of all order and arrangement in the universe at large."[2]

The "mind" which Anaxagoras introduced into natural philosophy did not, it is true, prove acceptable to the more critical thought of Plato and Aristotle; they both found this "simple", this "almighty and omniscient"[3] cause to lack value as a principle of scientific explanation. The criticism to which these more mature "teleologists" subjected the *nous* of their predecessor was not, however, directed against the implication of a guiding purpose in nature, nor even against the dualism which separated the teleological nature of the *nous* from the ateleological nature of the physical elements on which this *nous* operated as a force. On the contrary, it was because the "reason" of Anaxagoras turned with his handling of it into something too like a physical force, too like a mere *starter* of motions, that these later and more thoroughgoing teleologists found themselves disappointed in their hopes of an otherwise sympathetic forerunner. The "mind" that Anaxagoras had conceived disappears; his dualistic division of *mind* from *matter* remains. Numberless passages in Platonic metaphysics and ethics assume the dualistic hypothesis to account for the nature of man, for the character of the universe as a whole. Plato introduces a distinction between the "rational" and the "irrational" soul of man in such wise that "rational" conduct would exhibit the control of the rational soul, while what we today call "instinctive" behavior would respond to the influences of the

lower, irrational soul, the seat of all emotions and organic cravings. Although Plato recognized that the conduct of life represented the resultant effect of these two souls contributing each a component influence, yet the sharp distinction he drew between the "rational" and the "animal" in man initiated a tendency later carried on by Aristotle, by the Stoics, by Scholastic theology, and persisting down to the present. It is the tendency to recognize in *instinct* an attribute general to life, to restrict *reason* to a faculty peculiar to man.

At the same time, it is to be remembered, that with whatever "dualisms" Aristotle's philosophy may end, it begins with a perception of continuities of the first importance to later genetic conceptions of the mental life. His deepest thought sees in nature an upward striving exhibiting itself in an unbroken series of forms, from the lowest kinds of spontaneous motion to the highest form of terrestrial life, the human.[4] And although Aristotle's own thought is unable to complete this picture of nature as a triumphant upward striving, without introducing into the "highest form of terrestrial life" a certain "active reason" that separates it in kind from all lower earthly forms and makes it akin to a like "active reason" that activates the universe, it is not surprising that later thinkers should have come to look upon Aristotle's dualism as more or less accidental to, superimposed on, the essential motives of a teleological philosophy of nature. Thus, Platonic and Aristotelian world-views, if they literally support the later dualisms that invoked them as authority, could nevertheless suggest motives that might, and actually did, inspire later monisms. Meanwhile, the historic way to these later developments lies through the two late schools of classic reflection: the Stoic, which may be said to have "controlled" the teleological method of Platonist and Aristotelian with certain motives derived from Leucippus and Democritus; the Epicurean, which may be said to have reversed this order of emphasis.

In Stoicism, the relation between "rational" and "instinctive" factors in conduct becomes a special problem to be discussed in connection with one of more general import, the relation of all purposeful behavior to the all-pervasive mechanism of nature. For the Stoics, "all bodily movements are actuated by physical

causes,"[5] and all processes purposive in their nature (as is instinct) are, though teleological, none the less physically explicable. Since as physically explained the instinct can operate only under a law of physical order, yet in its results is seen to accomplish purposes, there developes a conception of unconscious purposive processes acting in consistency with mechanical forces. This conception, however vaguely introduced, promises ultimately to replace those earlier "spiritual principles" whose function it was to violate mechanism. So understood, its introduction may be regarded as a first step in the direction of monistic philosophies; although the Stoic system, as a whole, is generally taken to be of the "idealistic" or dualistic type. In particular, for Stoic as for Aristotelian thought a very valid distinction exists between "unconscious" and "conscious" reason, (though the terms themselves are of much later origin). Of these contrasting forms of "reason", the latter appears only in man, while the former manifests itself in various degrees throughout non-living nature, and in living nature from the lowliest organisms to man. Among the manifestations of this "unconscious reason" or general purposiveness, pervading the inanimate as well as the animate things of nature, is to be included what we should now call "instinct". The characteristics ascribed to instinct by the Stoics are not essentially different from those ascribed to it by most moderns; namely, its congenital character; its generally purposive function; its frequent persistence in situations wherein it acts to no purpose.

Epicureanism takes an attitude toward animal psychology very different from that assumed by Aristotelian and Stoic,—a change of view-point especially important because of its influence upon subsequent writers of antiquity, Plutarch, Celsus, Porphyry, themselves the great forerunners of the English and French Enlightenment. In the Epicurean system the Aristotelian and Stoic distinction-in-kind between the mentality of the lower animals and mental endowment of man gives place to a difference-in-degree. Under the influence of this thought, Plutarch, Celsus, Porphyry find a new interest in the study of animal psychology. For whereas Aristotelian and Stoic alike have approached the subject of animal life with a view to emphasizing the contrast between

animal and human mentality, Plutarch minimizes the distinction between man and the lower animals, accentuates the lack of any fundamental difference in their mental faculties. The complex adaptations of means to end, observable in animal behavior, he attributes to a wisdom equal to their contrivance. He recognizes the possibility of such metempsychoses as Pythagoras and Empedocles had assumed, wherein there is between man and brute an interchange of souls; he considers animals subject to those aberrations and fits of madness which Aristotle and the Stoics had denied to all below the level of "rational animals"; he accords to animals a moral disposition and a moral sense. Celsus goes even further in his estimate of sub-human mentalities; he dwells on the prescience of animals; on their special adaptability to environmental change, thanks to their intelligence and superior quickness. In Porphyry, this tendency runs to its extreme; he finds animals to possess all human faculties, sense perception, memory, imitation, speech (he thinks men to have understood the twittering of swallows), power of logical inference, aesthetic appreciation, et cetera. Along with these human qualities, Porphyry would have animals to share in all human vices and virtues; and from the occurrence in animals of delirium and madness, would argue that only as possessors of reason could they ever come to lose it. Animals, finally, are not without a certain religious sense.

Scholastic theology, Renaissance humanism. The philosophy of the Middle Ages contributed little to the clarification of the concept of instinct; neither did the Renaissance develop any new depth of insight. The scholastic conception of animal activity stands in a mean position between earlier psychologizing tendencies which ascribed rationality to animals, and the more modern mechanistic tendency which attempts to account for animal behavior entirely by physiological processes. Man, in his position midway between earth and heaven, is related to the lower animals in his bodily organization (with its equipment of sense, imagination, and appetite), and to God in his possession of intellect and moral sense. The Renaissance is best represented by Montaigne. The *Essays* reproduce the views on animal intelligence of Neo-Platonic and Neo-Pythagorean doctrine. The un-

scientific character of his comments on the animals appearing in his "stories" is familiar to all. Yet these stories are noteworthy because of a very definite influence they exercised on certain writers of the Enlightenment, and on popular conceptions of animal intelligence in general. Of their great variety, most serve to illustrate a superior intelligence in animals.[6]

"The Swallowes which at the approach of spring time we see to pry, to search, and ferret all the corners of our houses; is it without judgment they seeke, or without discretion they chuse from out a thousand places, that which is fittest for them, to build their nests and lodging? . . . Would they (suppose you) first take water and then clay, unlesse they guessed that the hardnesse of the one is softned by the moistnesse of the other? . . . And now useth one, and then another knot, except she has an imaginary kinde of deliberation, forethought, and conclusion?" To ascribe such behavior to the working of mere instinct, "with a kinde of unknowne, naturall and servile inclination," is unreasonable. . . .

"The Fox, which the inhabitants of Thrace use [to test the ice on a river before crossing, which listens to the roaring of the water underneath and so judges whether the ice is safe or not]; might not we lawfully judge, that the same discourse possesseth her head, as in like case it would ours? And that it is a kinde of debating reason and consequence, drawen from naturall sense? 'Whatsoever maketh a noyse moveth, . . . whatsoever is liquid, yeelds under any weight?'"

There are other "animal stories" showing, some the moral and religious sentiment of animals, some the nobility of animal nature.

At this point in history, one may well feel two problems of natural philosophy to have been as clearly posed and as thoroughly exploited as the technique of ancient, medieval, and renaissance thought permitted. Or rather, there has been but one problem, now presenting itself in its most general form, now discovering itself in special application: How can one and the same world be so thoroughly subject to laws of no purpose; yet so replete with forms that are nothing if not purposeful? And in particular, how can these purposeful forms, at one extreme of their ordered natures approach so close to an appearance of purposeless mechanism; at the other, reveal such mastery of this same mechanism as to suggest kinship to a divine intelligence?

It is only with this latter and more special question that the present study concerns itself; but it could not thus confine itself did it not assume the general problem to have found the beginnings of a solution elsewhere. So, one feels, with historic development as a whole; it can consider problems of psychology only in the light of a presupposed biology, a biology itself adjusted to the postulates of a presupposed physics. But to develop the whole of natural science in this methodical way, requires a careful technique in defining; for issues can be no clearer than the terms in which they are drawn. At the end of the Renaissance, a psychology run to story-telling betrays the exhaustion of the then available technique, reveals itself impotent of further development until the whole method of defining in the sciences on which a psychology must depend for its own first terms shall have undergone a thorough revision. To have undertaken this revision, to have begun its new approach to science with a "Discourse on Method," to have continued on its way with this problem of method, of ordered defining, of the adequacy of evidence, always before it—this is the great contribution of the Seventeenth Century to the development of science in general, of psychology in particular.

The Seventeenth Century. One might be content to call the history of this Century, so far as its science is concerned, the story of the Cartesian philosophy and its constructive critics. The very definitions in terms of which the problem with which we are here concerned presented itself to later thought, are framed in accordance with a method of defining imposed by Descartes' conception of a *rational* philosophy. This method gave first place to the formulating of such definitions and postulates as presupposed nothing but the idiom of logic and mathematics; these, it plausibly supposed to be the definitions and axioms of mechanics. A biology, then, to be built on these foundations, could begin in no other way than by asking itself how the thing it is willing to call *living* stands related as to its physical structure and operation to the underlying mechanism of all things, *i.e.* of nature. A psychology, if that is to come next, can begin in no other way than by considering how *living* mechan-

isms may be conceived to differ in those particular ways that lead us to distinguish the genus *life* into higher and lower species. How attribute to these mechanisms such different "faculties", and such different perfections of faculty, as ordinarily serve us to "account for "their observed differences of biological adaptability, of human efficiency? Descartes, himself, developing this vast program of scientific defining, almost succeeded in abiding by the presupposition with which he started; namely, that a scientific view of nature ought to be able to picture all that goes on in nature as consistent with nature's all-pervasive mechanism. Descartes *almost* succeeded; so nearly so that the very point at which he failed served as the starting point for that constructive criticism (of Spinoza, of Leibnitz) which made the end of Descartes' century such a worthy culmination of its beginning.

The general principles of Cartesian natural philosophy and their relations to classic models are too well known to require more than summary reference. Descartes restored to the position of a guiding principle of science, the Democritean ideal of nature as continuous, unbroken mechanism; only failing (as Epicurus and Lucretius had failed in like situation) to abide by his own postulates at that critical moment at which one would reconcile with the unbroken mechanism of nature the apparent freedom of man. Of organic bodies below the human he is able to draw a plausible picture wherein appears neither entity nor law not found in the inorganic world. With the single exception of the human, life is pure mechanism, the animal a peculiar machine. The whole realm of inorganic nature and of organic below the human, forms one material system in which mechanical causes alone are operative: plants and animals differ from inorganic bodies only in kind and degree of mechanical complexity. In "free" man, however, a non-mechanical influence, emanating from a mathematical point in the midst of the human mechanism, exercises a disturbing influence on the mechanical "atoms" in its vicinity. At this point the laws of mechanism are broken; a "rational soul" has its way; this way is *mechanically* free, *purposefully* directed.

In the end then Descartes is obliged to content himself with a "mixed" image of nature as a whole; and his system acts on

later thought as a challenge. This mixed image requires for its construction two essentially different kinds of point; the one, points of mechanical description and subject to mechanical law; the other, points of teleological description, making rather than following laws. To simplify, to purify this dualistic imagery, there could be but two ways. Of these ways, the one would eliminate the teleological (soul) points, and construct its image of nature out of ateleological (mechanical) points alone. The other would eliminate the (strictly) mechanical, and compose its image of (elementary) soul-points alone. The first was the way of Spinoza; the second, that of Leibnitz.

Each of these ways opens to the imagination several possibilities consistent with its general tendency and direction; as might be expected, the simplest solution is in each case the one first to to be thoroughly exploited. If one is to think of life in all its manifestations, not excepting the freest and most purposeful acts of man, as point-by-point obedient to the laws of mechanism, the easiest way is to attempt a definition of the class *living being* in terms of mechanism; the biology and psychology built on such definitions must prove of the order called *materialistic* (or, later, *mechanistic*). If, on the other hand, one is to think of the extended world as at some or all points free and purpose-working, the way is easy to a conception of *the living* as a class of purposeful beings, the integral purpose of each being in a sense later to be sought a "resultant" of the purposeful acts of its constituent points; the biology and psychology built on such foundations will be of the order called *vitalistic*. We may profitably follow in turn these two developments; their motives, all unformulated, are now to be recognized as having played their opposing parts in the historic moments of the past; their precise definition was hardly to have been undertaken until the technique of the Cartesian philosophy had furnished subsequent science with terms and images adequate to drawing the issue; adequate, that is, to establishing the essential contrast between *mechanistic* and *vitalistic* presuppositions, whether in biology as a whole, or in psychology in particular.

Materialistic developments. The story of modern materialism, although it promptly falls in with the history of post-Cartesian discussion, begins with certain contemporaries of Descartes who

presented their thought in their own idioms; Gassendi, Hobbes, Borelli.

To confine ourselves to the most systematic of these materialisms, we find in the *De Corpore* of Thomas Hobbes[7] a well-defined program to work out a biology and a psychology consistent with a mechanical definition of the organism; a definition then that must hold for *life* throughout all life's manifestations. For Hobbes, matter and motion are the only realities. Spirit is a motion of certain parts of the organism. Sensation and perception are movements within the organism; a physical impression made upon the sense organ, thence transmitted to the brain and thence to the heart, causes a sensation. The principle that every action is followed by a reaction applies to organisms as well as physical bodies. The reaction from the heart results in sensation and motion. Organisms react to pleasure and pain; and the activity of an organism is free when it originates with the organic tendencies of the individual and is unimpeded by outside forces. The difference between man and animals is one of degree, not of kind. Man is superior to brutes because he has the capacity for general ideas, and the implement of language. The association of ideas in man is purely mechanical; memory is but a continuation of a sense-impression.

Hobbes' type of materialistic psychology had only to be freed from all dependence on the idiom of psychology to bring it within the requirements of the most extreme mechanistic "behaviorism" of our day. This *purification* of materialism was undertaken by the Italian physicist and mathematician, Borelli. In his book on the motion of animals, Borelli[8] elaborates a theory of neuro-musucular activity along purely mechanical lines. The glands, too, operate in a mechanical way to effect a process of filtration, the varying calibre of the pores determining the separation of particles. But we need not follow here the further working-out of the materialistic motives, now so clearly defined.

Vitalistic developments. Turning from the mechanistic to the vitalistic development in biology and psychology, we recall Leibnitz formulating a system of metaphysics whose central conception was a "self-activity" analogous to "will". Such willfulness, when made to reside in the very elements of nature

(*monads*), must render the behavior of these elements, singly and in combination, indeterminate in terms of any mechanical law. The resultant conception of *life* must reduce the materialistic assumption, that the body of a living being is composed of the elements of a mechanical medium, to a contradiction in terms; and we understand any conception of life which carries this implication with it to be by definition a *vitalism*. Only on such a vitalistic biology could have been erected certain psychologies, in particular, certain theories of instinct and intelligence, that promptly followed on the spread of Leibnitzo-Wolffian influences. Among these psychologies, the one that offers most to the student of the history of instinct-intelligence theories, is that developed by Hermann Samuel Reimarus.[9]

In Reimarus we find animal activities to be the expression of definite inborn instincts, themselves the most convincing evidence of a general teleology in nature. The importance he attaches to the innate character of instinct, makes him a forerunner of those later antagonists of Lamarckian biology who specifically objected to its theory of *acquired* instincts. In support of the non-acquired character of instinct, Reimarus offers many observations of his own, touching on every aspect of animal life supposed to reveal the psychological accompaniment of instincts, their delayed and seasonal appearances, their resistance to modification during the life history of the individual. Indeed, it is only in his theory of the divine origin of this non-acquired equipment for life that Reimarus reveals himself a reminiscence of the past, rather than an anticipation of the future. Save for its theology, his biology would be similar in outcome to the natural-selection theories developed after Darwin.

To illustrate, Reimarus admits a gradual advance from the lowest animals to man; so that the highest animals are not far removed from man in sense perception and intelligence, although in them natural impulses and instincts take the place of reasoning and reflection. These impulses and instincts provide for activities, which, if they were performed by reasoning, would point to the possession among the lower animals of an intelligence surpassing that of man. They are present at birth, and neither improve nor decline; for they are established as perfect by nature.

Possessing reason, man makes up for the lack of those numberless instincts which are the endowment of animals (for man possesses only the sucking instinct, or but few more); and, by the exercise of certain undetermined potentialities for developing knowledge and skill, is able to lead a happy, moral, and highly intellectual life. He is thus able to make endless progress in knowledge and power, while an animal is confined within the limits of his original instinctive capacity.

If one stood, with Reimarus say, at the middle-point of the Eighteenth Century; one might experience once again that feeling which came at the end of the Renaissance; the feeling that the philosophic technique with which the deeper problems of natural philosophy could, at that time, be treated, had exhausted its possibilities. One might number oneself with one of two parties we have seen to develop out of critical reflections on Descartes. The one had begun by accepting the Spinozistic postulate, that nature could produce nothing that violated her underlying mechanism; and having accepted this postulate, what was left for the school to imagine in the way of a biology other than a materialistic or mechanistic theory founded on the assumption that since all things are mechanisms, the class of things called *living* must be susceptible of a mechanical definition? Such a theory could only proceed to multiply the number of phenomena observed to occur in living beings, whose explanation was to be found in laws of physical and chemical science; this it could easily do, but it could not by these methods advance a first step toward finding a meaning for those teleological terms which all men at all times had been forced to use in their simplest description of living behavior. The other Post-Cartesian school had begun by accepting the postulate of Leibnitz, that nothing nature had to show in the way of mechanism could discredit the universal opinion that life was purposeful and more or less free; while nothing could be free as a whole that was determined at every point, nothing teleological as a whole that was ateleological throughout. The *vitalism* which alone seemed consistent with these postulates might proceed, and did proceed, to multiply observations of life and of living behavior which could be classed

as of like teleology, but alike in no other way; it could not compose out of the *vital* points of its final assumption an image of a natural system subject to those invariable laws which all thinking men had come to assume, in their manner of describing the fall of a stone or the orbital swing of a planet. Without the invention of some new philosophic technique, the two parties could only strive to outface one another, each repeating its formula, each multiplying illustrations that, if they went to support the one doctrine, could do nothing to destroy the other.

The Critical Philosophy. As a matter of fact, before the Eighteenth Century had closed on its warring materialists and vitalists, that new philosophic technique which alone could advance the argument had been fairly developed; it was the technique of Kant's *Critical Philosophy.* All that was new in the method of Kant, cannot find room for clear statement in this study; of all that was new in its result, the point of most concern to biological theory may be put in this way: The categories of mechanics are *necessary* to the definition (and so, the recognition) of *life;* they are not *sufficient* to this end. The import of the first clause is, that had one not assumed a given object to be part of a mechanical system, one could not have recognized it as an *object*, whether living or not. The second clause implies, that had one discovered all there was to be known about a given mechanism, one could not in the least tell whether it was *living* or not. Equipped with the categories of mechanism alone, none could distinguish the living from the dead; to the recognition of life is absolutely indispensable the teleological categories, *means* to *end.* The first clause places Kant in permanent opposition to Leibnitzian and all other vitalisms; the second, separates him from La Mettrie, Holbach, and all later materialists. To the question, How is it possible for a being whose nature is teleological, to be part of a mechanical system?, Kant gives the clearest expression in his famous "antinomy of the faculty of judgment". Unfortunately, his technique is not adequate to the answering of this question in terms that have meaning for experimental science. An answer that can have meaning for none but an experimental science, will find its place among the "postulates

and definitions" on which the present study is based (Chap. II). It furnishes the starting point for a biology that can be neither vitalistic, nor mechanistic; yet can be nothing but experimental.

Evolutionary Theory. Meanwhile, the question which Kant so clearly posed, and so vaguely answered, is susceptible of being put in a way that seems less general, but is no less searching, than the form given it in the "antinomy". If one ask not how can (teleologically defined) organisms *be*, but how can they *come to be*, in a mechanical system with a long history behind its present moment, the more general question addressed to a space-time world is restricted for the moment to a more special one that confines its interest to the time-order alone. It will perhaps be felt that the special question can receive no ultimate answer that shall leave the general one unsettled; but it may be felt no less that an attack on the more restricted issue is a most promising way of approaching the more general. And it will also be seen that a problem analogous to the one just formulated for a (let us say) Post-Kantian biology must present itself, in the idiom appropriate to each, to materialist and vitalist alike. The mechanist who, following the oldest as well as the newest cosmogonies, recognizes an azoic period in the history of a system that now exhibits manifold types of "living mechanism", cannot escape a wonder as to how in one and the same mechanical system these heterogeneous and complex forms of mechanism should have come to follow on those homogeneous and simple beginnings. The vitalist who is not willing, with Reimarus, to let his biology partly depend on, partly support his theology, cannot avoid the question, How can living forms now so highly organized be resolved by imagination, traveling backward in time, into beings of simpler and simpler organization, until these become empirically indistinguishable from the elements of a mechanical system? In a word, as the Eighteenth Century passed into the Nineteenth, there could have been no more fruitful form in which to restate for further study the old problems of life and mind, than that which should shift the emphasis of its curiosity from a general question of the *being*, to a special question of the *becoming* of life, with all its gradation of species. And, in fact, one finds the further history of

biology, and of a psychology dependent on it, to be indissolubly bound up with the Nineteenth Century development of *evolutionary theory*. This theory, with its varieties, and their points of contention, is matter of such recent history and of such current knowledge, that its bearing on our topic is better reserved for discussion in the chapter on method to follow, than given a place in this one, whose story was to be of the past.

II

Postulates and Definitions of an Experimental Psychology

The value to one's method of a historical retrospect such as that which formed the matter of the preceding chapter lies in the confidence with which at its conclusion one may approach the classification of the contending motives that have underlain the debates of the past. Of these debates none have been so exclusively psychological in topic but that the motives impelling the contenders to their respective sides are traceable to identical origins, playing analogous parts in biology. If then one would have one's psychology profit by the dialectic experience of the past, one must first be sure that one's biology has already profitted by this experience. It must have overlooked no moral to be drawn from a *classification* of the grounds-of-difference that have divided schools of historic opinion concerning the meaning of *life*.

Biological conceptions. Our own essay at a classification of schools, to remain simple and yet prove exhaustive, must assume at the outset the right so to interpret the accidental variations of historic idiom as to justify the placing of all biological opinion prior to Kant in one or the other of the classes defined in the following propositions: (1) Every opinion is either *well-defined* or *ill-defined*. (2) Every well-defined biology is either *mechanistic* or *vitalistic*, and none is both.

To make this summary statement true, however, the meaning attached to each of these titles must be kept strictly in accord with the following definitions:

A *mechanist* is one who, (1) accepts (what we shall call) the *mechanical postulate*, "All that happens in a natural system must be consistent with a representation of that system as one of unbroken mechanism;" then, (2) from this postulate draws (what we shall call) the *mechanistic inference*, "No class of beings existing in a mechanical system is susceptible of definition in non-mechanical (*e.g.* functional) terms.

A *vitalist* is one who (1) accepts the *teleological postulate*, "There exists in nature at least one class of beings (*the living*), which class is susceptible of no definition in mechanical but only in functional terms; then (2) from this postulate draws the *vitalistic inference*, "No natural system containing functional beings is susceptible of representation in an image of unbroken mechanism."

From these definitions it will readily be seen, and must constantly be borne in mind, that

(1) To have accepted the "mechanical postulate" of the first definition is not enough to make one a mechanist: Spinoza and Kant both accepted this postulate; and yet Spinoza is not definitely, Kant is definitely not a mechanist. To become a mechanist one must draw from this "mechanical postulate" the "mechanistic inference"; the inference, namely, that a class of beings to exist in a mechanical system must as a class be susceptible of mechanical (non-functional) definition. The classic materialist (*e.g.* La Mettrie), the contemporary mechanist (*e.g.* Loeb) are biologists of *our* mechanist school.

(2) To have accepted the "teleological postulate" of the second definition is not enough to make one a vitalist: Aristotle and Kant both accepted this postulate; and yet Aristotle is not definitely, Kant is definitely not a vitalist. To become a vitalist one must draw from this "teleological postulate" the "vitalistic inference"; the inference, namely, that no being of a class susceptible of functional definition only can find representation in a mechanical image. The classic monadist (*e.g.* Leibnitz), the contemporary vitalist (*e.g.* Driesch) are biologists of *our* vitalist school.

The foregoing definitions and illustrations of the two classes into which must fall all *well-defined* biologies prior to Kant, reveal at once the meaning our scheme of classification attaches to the adjective *ill-defined*. An ill-defined opinion is one that has accepted either the mechanical or the teleological postulate; but, to the best understanding of the interpreter, has neither accepted nor rejected the usual inferences. Such ill-defined biologies might, with favorable interpretation, be regarded as consistent with the Kantian thought to which we now turn. But this Kantian thought is not itself ill-defined. It is, on the contrary, a new type

of *well-defined* biology, for it rests on the acceptance of both the mechanical and the teleological postulates, but is explicit in its rejection of either the mechanist or vitalist inference. The result is a composite postulate with which all biologies posterior to Kant must reckon; it may be put in this way:

(1) No object can be observed to exist in a natural system, whose supposed existence in that system would make it impossible to construct a mechanical image of the system observed.

(2) Nevertheless, objects may be observed to exist in a natural system, which objects are members of a class susceptible of no mechanical, but only of functional definition.

To distinguish this postulate, accepting both the *mechanical* and the *teleological* postulates, from either accepted separately, it will be convenient to refer to the Kantian assumption as the *mechanico-teleological postulate.*

One step more completes a classification of historical (and apparently of all possible) postulates of biology. Systems accepting the mechanico-teleological postulate may again be divided into two classes, according as their defining is in harmony with the principles of experimental method, or, on the contrary, introduces terms whose objects purport to be beyond approach by experimental methods. It is notorious that Kant, in attempting to show how both members of the mechanico-teleological postulate could be made consistent with one another, found himself obliged to refer to a non-empirical entity, the *noumenon.* The biology on which our present psychological study would depend for its terms and plan of procedure is definitely of the "experimental" type; whose fundamental postulate may be put in the formula:

No *term* can be said to be *defined*, unless the existence or non-existence in a natural system of a thing of the kind defined can be established by experimental methods. (*Experimentalist Postulate*).

Definitions available to a biology that would begin by accepting the "mechanico-teleological postulate," and in the end would conform its defining to the requirements of experimental science have in part been formulated elsewhere. For our present purpose we need review no more than the terms defined and the order of their defining.

The peculiarity of this biological defining (on which our psychological defining must ultimately depend) lies in its construction of the concept of *life* out of certain combinations of two categories only, *producer-product; probable-producer*. Of these, the first is definable as a relation between "mechanisms" forming parts of a mechanical system; the second, as to its adjective *probable*, in terms of a well-known mathematical idiom, the calculus of probabilities. Out of these categories, applicable only to phenomena to be found in a mechanical system, and only to such systems as furnish data from which are to be calculated the requisite probabilities, may be framed definitions of the following terms: *functional class; means-end relation; reproduction; self-maintaining mechanism; self-preservative mechanism; mechanism, self-preservative by means of self-maintenance; mechanism, member of an individual species; mechanism, preservative of its species by means of reproduction.*

If, as we suppose, these terms may all be defined in the way proposed, there can be nothing left indefinite in the conception of a class defined as follows:

A functional class of mechanisms, such that each mechanism shall be

(1) self-preservative by means of self maintenance;

(2) member of, or product of member of an individual species;

(3) preservative of its species by means of reproduction.

And it is easy to show from the nature of its construction that a class so defined will be

(1) susceptible of no mechanical definition; thus fulfilling the conditions imposed on a class to which the *teleological postulate* is applicable;

(2) capable of existing in no natural system save one of which a mechanical image can be constructed; thus fulfilling the conditions imposed on a class to which the *mechanical postulate* is applicable;

(3) determined as to its "distribution" in a given natural system by (a) the identical experiments that serve the physicist to limit the "range of permissible mechanical images" of a natural system observed (physical experi-

ment); and (b) a calculus of probabilities applied to certain data furnished by these experiments (statistical experiment); thus fulfilling the conditions imposed upon a class to which the *experimentalist postulate* is applicable.

If then the class so defined also fulfils, as to us it seems to fulfill, all the requirements laid by the experimental biologist on any class of beings he is willing to recognize as *living*, we have here a definition of *life* differing from all traditional definitions, yet based on postulates of which each one is acceptable to one, unacceptable to others, of the historic schools. The definition is *non-mechanist*, in that it subscribes to the *teleological postulate; non-vitalist*, in that it subscribes to the *mechanical postulate; non-transcendentalist* (Kantian), in that it subscribes to the *experimentalist postulate.*

The peculiar interest such a biology has for the psychologist, lies in the ease it lends to a statement of what is meant by two questions the psychologist, even more than the biologist, is interested in answering. The first, is the question, How does life come to be in a mechanical system once devoid of it? The second, How does life come to develop higher forms in a mechanical system once restricted to lower forms? If we are not mistaken, no definition of life, and no definition of "higher" and "lower" as adjectives of life-forms, can so readily suggest the kind of science required to answer these questions as can the definitions just offered. Only a definition which puts a measured probability-of-production into the meaning it gives to *life*, is prepared to see in the *how* of the questions just asked, an equivalent to the *why* of this other question-wording, *Why is it more likely* that a certain class of mechanism should have come to be in a mechanical system once devoid of that class, than that no such class should have arisen in that system? Any theory which understands the demand for an "explanation" of *life* and of *mind* to be equivalent to a demand that the above question in *why* find experimental answer,—any such theory is an *evolutionary theory* of life and mind.

In including the category *mind* in the foregoing statement, we are of course anticipating that the definitions of any psychology

to be founded on these biological conceptions will justify the inclusion; in assuming so much, we are committing ourselves to a sequel that shall meet anticipations. Understanding this, we allow ourselves a reflection that goes beyond the argument: It is vital to an experimental psychology that the biology on which it builds shall have so conceived life as to make an evolutionary answer to the questions, *why life becomes*, and *why life develops*, the only complete answer such questions can receive. Therefore, it is well to consider for a moment the order of questions that a biology built around the foregoing definition of life is prepared to address to any evolutionary theory of the origin of life. Only on the assumption that the previous definition of every term entering into their texture had been accepted could the questions be put in the form in which they are here asked. So asked, and as one may think only *when* so asked, an answer to them seems, if not immediately forthcoming, yet well within the range of possibility.—

1. If without any restriction as to type of law or distribution of fact, one were set to designing a mechanical system of sufficiently vast complexity, would it be easier to exclude or not to exclude from this design the possibility that somewhere in its spread (*e.g.* in some part of its history) a functional class should appear?

2. If in this design, one had allowed to appear functional classes of various kinds, which of two types would be more likely to endure; a class whose "function" implied type-preservation, or one that could fulfil its defining function without preserving its type?

3. If the designer had allowed to appear species preservative of themselves, which of two types of self-preservative species would be the more likely to endure; one that was preserved by means of its individual members reproducing each its type, or one that could be preserved only by means of its members preserving each itself?

4. If a species is to be preserved by means of the "reproduction" of its members, and if the chances of an individual reproducing itself in type increase with the span of existence of each individual, which of two species would be more likely to endure, one whose individual members preserve themselves by means of

self-maintenance, or one whose individual members are preserved, if preserved, by the accident of environment?

Suppose now we recall the definition of *life* previously given, "Life is a class of mechanisms; each mechanism being self-preservative by means of self-maintenance; each, a member or product-of-a-member of an individual species; each, preservative of its species by means of reproduction." Perhaps the foregoing questions and the answers they suggest will reduce some of the mystery surrounding the traditional wonder, how life could have come into being in a mechanical system from which it had once been absent. We suppose it to be rigorously demonstratable that no contradiction is implied in the proposition that life *as here defined* has so come to be. We now ask, Does not the evolutionary point-of-view (*i.e.* the application of a calculus of probabilities to the data of experience) make it appear less unlikely than has sometimes been supposed that life *as here defined* should so come to be; and, having come, endure?

Psychological conceptions. These preliminary reflections on a presupposed biology have seemed indispensable to an understanding of that psychology within which the topic of the present study must be regarded as a special, though very fundamental problem. For, in the first place, the conception of psychology from which we approach any particular problem of that science depends on the assumption that the categories of psychology are (1) irreducible to any required by the science of biology, but (2) susceptible of application only to a world in which certain biological conditions are fulfilled. It is at least apparent that a science could be imagined whose task it was to arrange under the fewest possible categories the ways (or a certain class of all possible ways) in which types of life might differ, when on the differences thus classified would be seen to depend the relative level ("higher" or "lower") of the lives compared. Evidently, a biology does not presuppose a science of the kind imagined, no matter in what way this science might later give precision to certain terms purposely left vague in the first suggestion of its task. For a biology could be developed and applied to a world wherein life on a certain level had indeed "emerged," but wherein

no lives of different level had as yet "evolved". To such a world the science just imagined could have no application; its *categories of comparison*, would be left with nothing to compare. From consideration of this order, one would be led to avoid any use of the terms developed by this comparative science, (which we shall provisionally call "psychology") in the posing or solving of biological problems. At the same time, while a biology may never *presuppose* a psychology, any evolutionary biology might well *anticipate* value in a science of life-comparisons. Its calculus of probability might well bring it to see that a world at one moment adequately described in terms of physics and biology alone, would *most likely* come to need a psychology as well. But from what empirical data could it be calculated that a world in which life had "emerged" must be a world in which life would "evolve"?

The question is more subtle than it sounds; and the puzzle of giving it an answer will be found to involve many of the puzzles of historic evolutionary theory. One feels at once that much depends on the sense one gives to the "need" of science in this matter: does science *need* categories of a certain order as soon as it is permissible to develop and convenient to apply this order of category? Or does it not *need* these categories until it shall have convinced itself that some problem of explanation can be carried to no solution until classification under these new headings shall have been effected? In a word, is the need of a science for new categories proved, the moment such categories are shown to be *permissible;* or are these permissible categories not really needed until they shall have proved themselves *indispensable?* A convenient, and also an historically interesting way of approaching this question would begin by dividing it into parts; and so we ask, first, What terms not to be found in physics or biology might science find it *permissible* and convenient to develop in classifying the relationships that distinguish higher life from lower? second, Which of these new categories would science find *indispensable* to explaining the evolution of the higher out of the lower?

Psychology as a descriptive science. In answer to the first question, What is the function of a descriptive psychology?, we may

point to studies already available. These studies illustrate the general answer in terms of the special categories, *sensibility*[10] and *spontaneity*[11], the first pair of categories to develop in the historic attempt of science to distinguish a higher (animal) from a lower (vegetable) life. In these studies, *stimulus* and *response* are first defined as essentially teleological terms useful in describing and comparing the observable behavior of living beings. Next, the conditions are formulated under which a stimulus-response phenomenon would be characterized as an exhibition of *pure sensibility*, not of sensibility entering into some more complex exhibition of "mentality", such as *perception*. Finally, it is shown that the historic development of psychophysics (from Fechner on) has provided us with a way of translating an apparently biological phrase into an admittedly psychological equivalent: "to exhibit sensibility" in biology, is "to have a sensation" in psychology. This proposed identification of two phrases is much more than the rhetorical suggestion it sounds: it is an identification that could not have justified itself to an experimental science until the development of psychophysics had shown the "sensation" attributable to any being exhibiting sensibility to be a *something* standing in such relation to other like-conditioned *things*, as to permit the manifold of all such *things* to be ordered along two dimensions; namely, (difference of) *intensity*, (difference of) *quality*. Only that to which can be attributed the properties "quality" and "intensity" can be included under the historic category of sensation; only if that which we say an organism *has*, when what it *does* is to exhibit sensibility, can be experimentally ordered along axes of quality and intensity, have we attained one object of an experimental psychology: we have placed this science in position to return an experimental answer to all meaningful questions respecting the distribution of *sensations* in a natural system.

This is not the place to elaborate on this simple illustration, to consider in what way the more complex categories that traditional psychology has grouped under the head of *knowing* (perceptions of various kinds) might be brought within the domain of observable objects. Let the very simple form of "knowing" called "having a sensation" suffice as an example of a psychological

category, not reducible to any biological category, convenient to the distinction of life levels, yet *not*, so far as we have shown, indispensable to the explaining of any phenomenon inexplicable in other terms. From the point of view of explanations, any picture we may care to draw and experimentally verify of a living being's sensation-content is by way of portraying an "epiphenomenon". To picture the distribution of "sensations" in a natural system may involve an interesting, even an economical art of image-making; as such, it may serve to compare lives in terms of their mentalities; nothing as yet suggests that it is an art without which anything science is interested in explaining or predicting would have to go unexplained and unpredicted.

Perhaps in admitting so readily the "epiphenomenal" in sensation (and no doubt other cognitive forms of mentality) we have been willing to remain uncritical for the sake of the argument; we have been more willing to excuse than anxious to justify a well-known historical attitude toward the relation of body and mind. However that may be, it will be interesting to remain in this unquestioning mood long enough to formulate a query that bears directly on our special problem. Even if we regard psychology as a science purely descriptive, or *re*descriptive, of living phenomena whose explanations must be sought in biology, are there any categories other than those of *sensation* and *complex-of-sensations* which this science must find useful in its program of classifying "differences of mind"? Nor are we here suggesting, by way of answer, the obvious categories of volition; since the studies to which allusion has been made make it definitional that *sensibility* and *spontaneity* (in biology), *having a sensation* and *having a volition* (in psychology) are perfectly distinguishable but absolutely inseparable aspects of whatever behavior calls for the use of either term of description. As for the *emotional* life, its categories may for ought we can foresee be mixed with those which would supposedly develop in connection with sensory and volitional descriptions of mind. But there is *one* category of psychology that, even if psychology remained a purely descriptive science, would need to be developed by that science for the sake of economy; the economy not so much of its descriptions, as of its gathering of observations sufficient to

verify these descriptions. It is, we should like to show, the category of *intelligence.*

The reasoning that would conduct one to this conclusion must begin by defining a little more carefully certain terms that have so far been taken for granted, as indeed they too frequently remain taken for granted at the end of biological and psychological discussions. They are the figurative terms "higher" and "lower", commonly used to distinguish "levels" of life. But the terms *are* figurative; and therefore they leave the experimental test of the differences they are supposed to connote, to the invention of the non-figurative experimenter. Of all the terms suggested by the experimental biologist as standing for that whose observable and measurable degree will best serve to order life-forms on a scale of levels, none is less unsatisfactory than the term *adaptability.* Only, of course, abilities of whatever kind are not directly observable; they can only be calculated from, or expressed as functions of, observable acts performed in observable situations. Now, there is an ideally simple case in which we might express all that is connoted by greater or less adaptability in an ideally simple formula. If of two organisms, A and B, A is calculated to succeed in accomplishing its defining purpose in $(n+1)$ situations; B in but n of these, A is said to be "more adaptable" than B. The "calculus" here involved is, of course, a calculus of probabilities; the data to which it is applied is the statistical result obtained from having watched A and B, or other individuals of the A-type and of the B-type. Obvious reflections will suggest the way of distinguishing between the evidences which would establish *better adaptation* on the one hand, *greater adaptability* (which alone concerns us here) on the other; those particular attributes of an organism, of which we should expect to find its degree of adaptability a function, are doubtless not its physical properties, but potentialities in the way of response. (*Stimulus–response* has been defined in the studies referred to, Singer 2, 3).

Now any potentialities of the kind described, whose advantage to the individual could be determined by the study of two individuals, one of which has succeeded in $(n+1)$ *situations*, the other in but *n of these same situations*, would be, or would yield, a category of psychology. Delicacy of sensibility, intensity of

sensation, are psychological terms whose proper attribution may generally be reduced to an experimental problem of this simple kind. But the practical application of such methods of comparison is obviously limited, and the science interested in the most general comparisons of life-levels, of potentialities of success, is bound to experience an economic need for an ideal attribute defined by no other requirements than those laid on it by the need itself. It must, like all others in which an experimental science can interest itself, be a *measurable* attribute; but in particular, it must be an attribute such that its measure can be determined by the study of any purpose-seeking activity its possessor may display, in whatever situation he displays it. Finally, it must be a property such that, if *its* measure be known, the measure of no other attributes need be considered in determining the mental level of its possessor. Or, what is a corollary of the preceding, the descriptive and classificatory psychologist is interested in determining other mental attributes of a living subject, only in so far as they are attributes of which this ideal determinant is taken to be a function.

If there is any historic category that, could it be empirically applied, would come nearer than another to fulfilling this need inherent in the very nature of psychology, the survey of the last chapter will probably suggest *intelligence* as that category. That there should be an important term of history whose construction comes fairly near to satisfying the need of an experimental scientist, is doubtless due to the fact that the "scientific need" in question is scientific only in the superior clarity of its formulation; the demand itself is none other than a human need universally felt, and so, historically illustrated. If then, *intelligence* be taken for a moment as the name of this ideal category, X, which a sufficiently reflective psychologist would certainly want to develop, then *this* "intelligence" would be defined as an experimentally measurable attribute of mind, such that the living being which revealed the greater intelligence would be "higher than" the living being which revealed the less; the difference in degree being susceptible of numerical statement. The question the present study would approach in the end, is the experimental question, Is there any experimentally measurable attribute of

living beings that fulfils the conditions laid on this ideal attribute, *X*, tentatively identified with the traditional attribute, *intelligence?* It is altogether to be supposed that any discussion of this ideal attribute must develop a term to be applied to its opposite; and if the ideal attribute turn out to be indeed the realization of such hopes as history has put into its use of the word *intelligence*, it is altogether likely the opposite of this ideal attribute will express, in a developed sense, all that history has *tried* to convey in its use of the word *instinct.*

Meanwhile, there remains to be considered the second heading of those two under which we proposed to discuss the need for a psychology in general, the psychological category of intelligence in particular. It has been shown how useful to a science whose task it was to develop categories in terms of which different levels of life are to be compared, must be the development of some single category in terms of which difference of life-level is to be *experimentally* measured. If as we have pretended, traditional psychology is the science that has undertaken this task, all these considerations attest the value of this psychology as a descriptive and classificatory science. But we should also like to know, if for no other than historic reasons, whether psychology is to be regarded as a science not only useful to the describing, but indispensable to the explaining of natural phenomena. In other words, we would know whether or not psychology is an indispensable explanatory, as well as a convenient classificatory science.

Psychology as an explanatory science. Any recollection of the history lately reviewed, will suggest at once a natural phenomenon in connection with which mind and its faculties has most frequently been invoked as a producer of results. Are there then phenomena that could not have been explained save as the product of mind in general, intelligence in particular?

Now if intelligence have no more than the function already imputed to it as a descriptive category, it must still play an important part in defining what is meant by evolution. Evolutionary theory is always, it has been said, an application of a calculus of probability to the explanation of an historic sequence; but

it is also understood that this sequence, in order to submit itself to the kind of calculus evolutionary theory has proposed, must be a "developmental" sequence. What then must a sequence "develop" in increasing measure, if the development is to be called an organic evolution? Evidently, a science would wish to give to *some* category a meaning that would make it the definitional answer to this question, and we suspect this category to have been already developed under the historic name, *intelligence.* This would make *intelligence the product of evolution;* but is there any reason to suppose that *evolution is the product of intelligence?*

A glance in review over the historic notes set down in the last chapter will suggest to one that there is no time so remote, and none so recent, but will have its share of theories ascribing the appearance of lives-at-different-levels to the action on the world of *some* intelligence, operative *some*where and *some*how. Aristotle is interpreted by most to have attributed the scientific activities of man to his share in an "active reason" closely akin to the "divine intelligence". Reimarus saw in instinct, "unlearned" by the being blessed with it, a quality of wisdom which could only have been bestowed by an Intelligent Creator. The modern evolutionary school of Lamarck is given to invoking an individual intelligence as indispensable to those individual modifications of response which, being inherited, develop into race habits preserved or eliminated according to the survival value they possess. The contemporary evolutionary school of Osborn, Morgan, and Baldwin suggests the combined action of accidental (*i.e. not* intelligently directed) variations, and "consciously directed" modifications, the former inherited, the latter not, to account for the observed phenomena of life in its graded development.

The responsibility of the present study toward problems of this order is at once simple and serious. It is, to perfect the definitions of the terms involved to the point of making it clear how much of the historic issue into which these terms enter is a matter of meanings, how much a question of fact; with the hope of bringing as much as possible of this issue under the control of experiment. In our own day the questions most troubling the evolutionist arise from the breakdown in presence of some newly considered fact of some theory of probability that has *up to this*

point made the development of higher life out of lower, an acceptable likelihood. Thus, it may seem probable enough that members of a class having only function, not mechanical structure in common, should in the course of their continually renewed production, under continually new conditions of environment, present variations of mechanical structure. It may seem probable enough that the relative adaptability of the different types of living mechanism should be a function of at least some of their structural variations. The "survival of the fittest" would then be a probability; and with the right interpretation of "fitness" ought to satisfy science that an evolution of life is more likely to take place than not to take place in any world where life of some kind is already present. But now suppose one to be suddenly faced with the fact that variations, to have any understandable survival value, must be not slight but fairly pronounced; not confined to one structure, but demanding the coördinated variation of many structures whose likelihood of simultaneous variation in the way demanded seems infinitesimally small. The puzzled evolutionist feels almost forced at this point to abandon his characteristic device of calculated probability in favor of an historic development, and to appeal to a "directing something" whose only definition is, to be "that which solves the problem it is invoked to solve." Naturally, with so much history to draw on, we are likely to call this "something" by some name the past has given to an *X* long ago invoked as a very present help in time of similar trouble; and it is fair to suppose the future will frequently repeat the performance of the present. Without renewing here the frequently expressed impatience of the experimental scientist with such "explanations" as these, we would nevertheless turn the long experience of history to the greatest practical profit. And the only practical profit to be drawn from history's eternally repeated misuse of the category of intelligence (to speak only of that) as an explaining cause would seem to be this counsel: "Intelligence should never be used to explain any phenomenon that has not been experimentally demonstrated to be a phenomenon intelligence can explain." In order to abide by this counsel, it is necessary first so to define intelligence, that the measure of it revealed in any given natural situation can be ex-

perimentally measured. It is such a definition this study has set itself to seek; and the demand for it on the part of an "explanatory" psychology imposes no other conditions on the form the definition should take than those already formulated in considering the ideals of a "descriptive" psychology. The two conceptions of psychology are indeed not different; the entire historic sense of difference between descriptive and explanatory sciences, lies in the different ways in which questions may be put to one and the same science.

We turn then to the construction of a definition of *an intelligence;* it may prove to be at the same time the historic category of that name, and the ideal category, X, demanded by psychological science.

III

Critique of Current Opinion; Preliminary Definitions

It would of course be possible to construct a definition subjected to such formal demands as those the last chapter laid upon a certain ideal category of psychology without any consideration of history. But the conservative thinker is loath to attempt constructions for which he has not demonstrated a scientific need. He is better satisfied if he can show this need to have been of long standing, and persistent. He is best content if he can show the discontent with which the past has viewed its own efforts to meet its own needs to have resulted from a definite reason. But reason for failure *is* definitely established wherever it can be shown that the motives for past offerings fall into classes of which each has failed to seize the significance of what the other has held most important. And where one finds current discussion continue to illustrate all the old motives of confusion, a double purpose is served in proceeding to new construction along what one might call an historico-critical way. We propose then to subsume the most recent expressions on the subject of *intelligence* and *instinct* under the principles of motivation classified in the last chapter; concluding the presentation of each class with as formal a comment as possible on the motive this class of definition will be seen to have overlooked. At the end of such a procedure, one ought to be in position to consider whether all the motives, whose too fragmentary acknowledgment has caused the confusions of the past, may not be accommodated in one "more experienced" definition. If this definition prove also to fulfil the formal demands previously laid upon a certain ideal category demanded by psychology, two mutually important tasks, that of the historically-minded critic, and that of the experimentally-minded builder, will have been simultaneously accomplished.

As has been said, the motives entering into psychological definition are identical in kind with those controlling biological defining. We shall expect then, if the present continue the story of

the past, to find the *mechanistic* and the *vitalistic* motives of biological thought determining the varieties of meaning imputed to psychological terms in general, to *intelligence* and *instinct* in particular. As in biological thought, so in psychological, the *mechanistic* bias will be recognized by its unwillingness to introduce into its defining, terms of teleological reference; the *vitalistic*, by its willingness to employ none but teleological terms. Our first comments will take the general form of a protest against the supposition that either ateleological terms or teleological terms alone can suffice to the defining of any category either of biology or of psychology.

Under *mechanistic* or *ateleological* conceptions of instinct (to begin with the simpler category) would certainly be classed such as define the instinctive act in terms of a direct sensory-motor mechanism (reflex) assumed to be always involved, avoiding all reference to any end the mechanically-produced reaction may on occasion subserve. The sensory-motor connections may be of relatively simple or relatively complex pattern; they may or may not be characterized as to their genesis. For Loeb,[12] the instinct is a "tropistic reaction"; for Watson,[13] "a combination of congenital responses unfolding serially under appropriate stimulation"; for Spencer,[14] "a compound reflex action"; for Hobhouse,[15] a "response of inherited structure to stimulus"; for Eimer,[16], "an inherited habit."

For convenience, we shall throughout the sequel, allow the simple expression, *type-of-act*, to stand for any observable gesture so named and described as to let its class be recognized without reference to purpose. Thus, a *tropism*, defined exclusively in terms of an environmental condition (*e.g.* source of light), and a non-functional relation connecting the act with this condition (*e.g.* movement toward or away from source), is an act described in a way that permits us to refer to it simply as a *type-of-act*. So, *reflex*, *behaviour-pattern*, and other terms entering into the foregoing definitions of instinct are readily to be recognized as *types-of-acts* in the ateleological sense of *type* to which for the moment we restrict its use.

Our *comment* on mechanistic definitions of instinct is then this: It may be that *instinct* is susceptible of no definition in terms devoid

of reference to an (ateleological) *type-of-act.* But no definition of *instinct* can be completed in terms of such types-of-act alone, avoiding all reference to *purpose* or *function.* Any type-of-act to be recognized as instinctive, must have been found "sometimes but not always" to serve a purpose, a purpose characteristic of some teleological class under which the agent must have been subsumed.

Under *vitalistic* or *teleological* conceptions of instinct, would doubtless be included all those that describe instinctive behavior as an innate "drive" or "set," working toward some "goal". The most consistent illustrations of this kind of defining are those recently offered by Tolman,[17] Dunlap,[18] Craig[19] and Woodworth.[20] These writers may differ in the descriptions they offer of "initiating stimulus," "drive", "goal"; they are at one in the acceptance of a teleological method of defining. Thus, the *initiating stimulus* is, for Dunlap and for Craig, intraorganic; for Woodworth and for Tolman, either intraorganic or external. But for all four alike, the *instinctive response* to this stimulus is to be described in teleological terms: the "drives" or "sets" involved are for Woodworth, "reaction-tendencies"; for Craig, "appetences" and "aversions"; for Dunlap, "fundamental desires"; and for Tolman, "determining adjustments"; while the goal of these drives or sets is, for Woodworth and for Craig, a certain consummation; (in Craig's "aversions", it is a simple "getting away from" the stimulus); for Dunlap, "a particular general type of end-behavior; or a particular type of general-end situation"; for Tolman, "a particular type of final rearrangement of organism and external object."

Our *comment* on vitalistic definitions of instinct can only invert the protest of the last, on mechanistic definitions. It may well be that *instinct* is susceptible of no definition in terms devoid of reference to the (teleological) *function of an act.* But no definition of *instinct* can be completed in terms of goal or purpose alone, avoiding all reference to an (ateleological) *type-of-act.* Any functional act, to be recognized as instinctive, must have been found "sometimes but not always" to fail of its purpose, a failure that would leave the abortive act unrecognizable as of the same *type* as the successful, were there not some ateleological class under which both must have been subsumed.

As in its definition of *instinct* so in its conception of *intelligence*, contemporary opinion reflects the old opposition of motives.

The *mechanistic* or *ateleological* conception of *intelligence* seems to have become relatively rare in contemporary discussion. It is, however, to be found illustrated in such definitions as that in which Peterson[21] takes intelligence to be "a biological mechanism by which the effects of a complexity of stimuli are brought together and given a somewhat unified effect in behavior, a mechanism for adjustment and control, operated by internal as well as external stimuli."

The more frequent *vitalistic* or *teleological* contemporary motive is illustrated in all those definitions which, in making use of such categories as "ability", "capacity," "process-making-for-success," employ terms whose connotation is definitely teleological. Thus for Terman,[22] "an individual is intelligent in proportion as he is able to carry on abstract thinking" (and Terman designs a hierarchy of intelligences, based on the difference-in-level of intellectual functioning); for Henmon[23], "Intelligence is indicated by the capacity to appropriate truth and fact, as well as by the capacity to discover them"; for Haggerty,[24] intelligence connotes no "single mental process capable of exact analytic definition," but is "a practical concept connoting a group of complex mental processes traditionally defined in systematic psychologies as sensation, memory, imagination, discrimination, judgment and reasoning;" for Freeman,[25] intelligence is *what is measured* by the so-called "intelligence tests" (and Freeman would include among the capacities to be measured in such tests, the neglected traits of "mental balance, coördination of the processes of learning or reflection, mental control, degree of non-suggestibility"); for Woodrow,[26] too, intelligence is that which is measured in the "measurement of all the complex processes which have a part in securing 'success' in general."

By way of *comment* on both classes of definition of intelligence, it is sufficient to remark that if, as one may begin to suspect, no definition of intelligence is possible which does not make *the intelligent* a correlative of *the instinctive*, then the need to employ both ateleological and teleological terms, experienced in the defining of *instinct*, must be no less felt in the defining of its correla-

tive, *intelligence*. But it is difficult, at this point, to suppress an observation suggested by the preceding definitions of intelligence; namely, this: It is not in the least helpful to offer definitions of intelligence in formulae that presuppose the definition of such other terms as "abstract thinking", "intellectual functioning," "capacity to appropriate truth," and finally, "talent". Of the definitions just reviewed, which may be taken as typical of current discussion, there is but one free from the vice of defining the difficult in terms of the more difficult, or at least of the previously undefined; and that is the suggestion of Woodrow which would make the measure of "intelligence" to depend on the "measurement of all the complex processes which have a part in securing 'success' in general". It will presently be noted that in our own final expression for *intelligence* and *instinct*, *success* is the one teleological term presupposed by the definition. It may reasonably be hoped that "success" is not only a term easy of definition, but that this definition will show it to be indeed such a "measurable process" as an experimental psychology demands for the exact statement of its meanings and problems.

This last reference to the demands imposed by an experimental science upon all definitions apt to serve it in formulating questions susceptible of experimental answer, recalls the last phase of our classification of biological motives. The Kantian or *mechanico-teleological postulate* was there said to have caught up and preserved both the *mechanical* and the *teleological postulates*, without having drawn from either those unwarranted *inferences* which have brought mechanist and vitalist into historic conflict. The foregoing comments on current opinion will already have suggested that in our own psychological defining we intend to build on just such a mechanico-teleological postulate as was Kant's; but we would now have it recalled that Kant himself was unable to complete in terms intelligible to experimental science the reconciliation of the two descriptions (mechanical, and functional), which he rightly held to be equally true descriptions of every living body at each moment of its existence; instead, he had recourse to the experimentally meaningless concept of the *noumenon*. As one might expect, an analogous non-

empirical note is preserved in current defining; and, noticeably, in the defining of those few psychologists whom we should be least willing to class as either *mechanist* or *vitalist*. These psychologists are, of course, exempt from the critical implications of our preceding comments on mechanist and vitalist respectively, but they are not exempt from the Kantian fault of introducing into definitions meant for the use of experimental science, terms referring to "facts" assumed to lie beyond the reach, or even approach, of many-sided observation. To these definitions of better promise, marred only by their reference to "non-experimental facts", this survey of current opinion in the light of past history may reserve its final note. Meanwhile, an interesting approach to this closing note on a certain "non-experimental" psychology, will be by way of a philosophy of our day even more remote from the experimental. Yet between these two non-experimentalist doctrines there exists a certain contrast; a contrast that developing as it does in their theories of *instinct* and *intelligence*, happily serves to recall an important ancient issue that has so far escaped our comment.

As we pass from the lowest grade of life to the highest, does the transition from the most instinctive to the most intelligent behavior observable at the two extremes present itself as a fairly continuous process, or are we met at a certain elevation by a sudden jump from a type of behavior to be called "instinctive" to another type, to be called "intelligent"? This is an issue whose antiquity is amply established by our summary review of history (cf. Chap. I); and one is not surprised to find it dividing, as do all sufficiently ancient issues, the current opinion of our day. No doubt the influence of evolutionary theory has been more and more to favor in biology and in psychology the substitution of gradual transitions for the more classic conception of suddenly appearing rifts, breaks, discontinuities. Nevertheless there is to be found among those of the day one influential writer who is inclined to insist upon what he designates as the "cleft of separation" that must forever divide the instinctive life, on the one hand, from the intellectual or intelligent, on the other. It is Bergson who thus departs from the trend of the day to revert to older models (though the more immediate derivation of his

thought might be traced to certain moments in the *Naturphilosophie* of the post-Kantian period). His peculiar position is here mentioned only, as was said, for the sake of recalling an historic issue and introducing a contemporary contrast; for the intuitive non-communicable character that Bergson attributes to our immediate experience of life can contribute nothing to a better expression of the difficulties that beset an experimental biology and psychology. The episode does however suggest one reflection of an historic order that may be of interest to the sequel.

Our *comment* on all such "dualistic" theories of *instinct* and *intelligence* is less unfavorable than our estimate of the non-experimental philosophies from which they often spring might lead one to expect. Ancient opinions, however indefensible their exact expression may have become in the light of later progress, are seldom without some motive that has lost none of its ancient validity. It will be a matter of satisfaction if, in the end, our definition of *instinct* and *intelligence* prove to have preserved the old suspicion that between these two there is indeed a sharp contrast *in kind*. Now, there are well-known cases in which a variable, passing as continuously as one pleases through a series of values, may define certain ranges of these values that stand opposed not in degree but in kind: one case is that in which the variable, forced to pass through the value zero, opposes a range of *plus* to a range of *minus* values; another, that in which a ratio, forced to pass through the value unity, opposes a range of *direct* to a range of *reciprocal* values. If the terms *instinct* and *intelligence*, already suspected of being *correlatives*, can be defined by means of some such correlatives as a ratio and its reciprocal, or a function and its inverse, an important historic conflict will have been justified by the preservation of both its opposing motives.

To a final expression for *instinct* and *intelligence* which shall indeed present the two as differing no less in kind than does a ratio and its reciprocal, or as a direct function and its inverse, we are most quickly led by following the suggestion of those contemporary views on the subject to which the Bergsonian dualism was to serve as introduction-by-contrast. The type of analysis we have in mind is one that *lets oppositions fall out of continuities*,

of which an interesting example is to be found in certain writings of C. S. Myers.[27] These proceed, as might have been anticipated, to consider not the contrast tradition has assumed between instinctive *acts*, and intelligent *acts*, but the distinction reflection would draw between an instinctive *aspect* and an intelligent *aspect* of one and the same act. "Each [of these aspects] is to be found in every act . . . Instead of defining instincts as 'complex reactions which are perfect the very first time', I have endeavored to show that they are within very variable limits improvable by practice or by imitation, or are modified by changed conditions of environment." The psychology and physiology of "instinct" Myers has taken to be inseparable from the psychology and physiology of "intelligence". There is not one apparatus for "instinct" and another for "intelligence"; neural prearrangements are present in instances of both kinds of activity; the limits of each are fixed in each individual and in each species. The two ought "to be treated as one indivisible mental function which in the course of evolution has approached now nearer to so-called "instinct", now nearer to so-called "intelligence". These two terms relate to different aspects of the same mental processes. "Regarded from the objective standpoint, instinct-intelligence appears as instinct; regarded from the subjective standpoint, it appears as intelligence. The obviousness and complexity of these appearances depend on the mental development of the organism which regards, and of the organism which is regarded."

With the exception of this last reference to "subjective and objective aspects" of living acts, there is much to recommend itself in Myers' estimate of what ordinary experience may be taken to show; namely, that no act regarded without reference to other acts, and to the context in which each act is involved, is to be recognized as either instinctive-and-not-intelligent, or intelligent-and-not-instinctive. But if this is so, there must be some peculiar possibility of classification, or of comparison based on classification, to justify us in recognizing in one act "a something more instinctive"; in another "a something more intelligent." We shall expect to make every use of this suggestion in the sequel; it is only when Myers is called upon to specify what

this "something" that distinguishes "aspects" of living acts can possibly be, that he falls into the "Kantian fault" from which we are anxious to separate ourselves: he appeals to a something less *non-empirical* than Kant's "noumenon" but no less *non-experimental;* the "intelligence-aspect" of an act turns out to be its "subjective aspect". "In place [Myers writes] of the usual definition of instincts as merely 'fixed innate activities', I have maintained that the essential mental concomitants of instinct are the feeling of activity and a vague consciousness of the behavior to be achieved; that the existence of these central factors proclaims the presence of "intelligence" throughout instinct, and that as the organism becomes endowed with an increasingly large number of mutually incompatible modes of reaction, the intelligent aspect apparently receded *pari passu* into the background."2

Our *comment* on these last developments of Myers' analysis, reflects the attitude of our earlier postulates toward every introduction of a "purely subjective" element into the definitions of an experimental science. It may well be that *instinct* and *intelligence* are substantives whose meaning is best seen in the adjectives from which they are derived,—*instinctive, intelligent.* It may well be, too, that these adjectives themselves are always tacitly comparative; that an "instinctive act" is one "more instinctive" than some other (to be identified by reference to the context in which the judgment is made); and that, *mutatis mutandis*, the same may be said of "intelligent acts". It may well be, finally, that *every* act is more instinctive than some other, more intelligent than some third. But the possibilities opened by these reflections are of no interest to an experimental science, until they shall have been realized in a definition of *the more and the less instinctive*, of *the more and the less intelligent*, such that an experiment susceptible of being repeated by more than one observer can establish the measure of every difference of degree therein defined.

It remains to be seen how far all the foregoing comments on historic and current opinion can be made to profit us in a final expression for *instinct* and *intelligence*. Plainly, their cumulative effect must be to drive one to seek for each and every category of psychology a definition that shall be (1) non-mechanistic; (2)

non-vitalistic; (3) mechanico-teleological, but also completely empirical; (4) completely empirical in the sense of completely experimental; *i.e.* admitting no "fact" to have been established at all, unless established by observations susceptible of being confirmed by more than one observer. In addition, and so far as concerns the particular categories before us, it seems likely that a definition of *intelligence* and *instinct*, to do full justice to all historic motives, must (1) derive these substantives from their corresponding adjectives; must (2) recognize both adjectives as applicable to every living act; yet must (3) keep such difference in kind between the *intelligent* and the *instinctive* as can only be imagined in presence of the continuities of living development by making each adjective a function of the same variables, but the one a *direct*, the other an *inverse* function.

Preliminary Definitions. Let us begin the construction of our final definitions by recalling an observation already made; namely, that an act to be regarded as instinctive at all, must be recognized to be of an (ateleological) type that is *sometimes* teleological. For example, one can see that while certain instinctive acts may be carried to such lengths as to be "tragically stupid", as is the "passion" of the moth for the flame; yet even these acts must have proved sufficiently preservative of the agent to allow a biological species given to such instincts to continue in their practice. Now, in the reflective observer's ability to distinguish the case from the rule, we seem to have a means of differentiating the instinctive act from the intelligent aspect of behavior. Our experience as observers has given us a fairly correct idea of the range of situations in which a given type-of-act is likely to be performed by a given agent. This total range of cases in which the act is to be expected, will generally include a fractional range within which the act would lead to "success"; and another, within which the act must end in "failure". Throughout that restricted range for which the act is well-calculated to succeed, its performance may be characterized by the reflective observer as "seasonable"; throughout the remainder of the total range, its performance can only be regarded as "unseasonable". "Instinctive behavior" developed in

season exhibits a complexity of adaptation of means to end that makes us wonder; persisted in out of season, it exhibits an equally marvelous neglect of the simplest possible re-adaptations. When, preparatory to lying down, the dog instinctively "makes its bed" in the tall and possibly vermin-infected grass, the act seems to us "seasonable", or as we more commonly say, "reasonable". So indeed it is, but when the same animal, or one of its species, goes through the same performance before a well-swept hearth, where grass is absent and the floor "biologically clean", the act appears to the reflective eye exactly the opposite of reasonable: it looks *irrational*. But is it not at precisely such moments as this, moments at which the observer feels called upon to explain the irrational performance of an act well-known to be *sometimes* rationally performed, that the word *instinctive* springs to the lips?

It will be seen that without departing from the uses of familiar speech, we have slipped from one to the other of the adjectives "seasonable," "reasonable", "rational"; from one to the other of their opposites "unseasonable", "unreasonable," "irrational". But from beginning to end no term has been employed whose applicability to any given case is beyond the reach of an observational test. And it must be equally an experimental matter to estimate the range of "occasions" (types of necessary condition) under which an organism might be expected to perform a given type of act (*e.g.* "moving toward", "circling", etc.). If on none of the occasions included in the estimated range could the type-of-act be recognized as rational, it would fall without our present interest completely; if on all such occasions, it would lead us to suspect a "divine reason" at work. But if, as is the case in all known instances of animal (including human) behavior, the total range was found to fall into two parts, a range of "rational performance" (R); and range of "irrational performance" (I), the whole situation might be made to define two terms convenient to our further discussion; namely,

$$\textit{Coefficient of rationality} = \frac{R}{R + I}$$

$$\textit{Coefficient of irrationality} = \frac{I}{R + I}$$

Evidently, any learning process in the course of which an organism was seen to curtail the range I, or extend the range R, or in any way to augment the ratio $\frac{R}{I}$, would be regarded as a growth of rationality on the part of the organism, but neither coefficient as it stands implies anything dependent on a learning process. The *coefficient of rationality* (to confine our remarks to it) is an abstract number, to which no dimensions are as yet attached; this number is available for measuring *something*, but whatever that something may turn out to be, it must remain descriptive of some momentary condition of the agent; it cannot connote a rate-of-change of this condition with a variable involving *time*. Consequently, we are not prepared to associate this coefficient with a constant of proportionality whose dimensions would confer on the whole expression, *constant* × *coefficient of rationality*, the meaning, *degree of intelligence*. Nor should we be in the least inclined to make this coefficient of rationality measure the *reasoning power* of the agent; the ability to recognize such reasonableness of performance as has been invoked in the defining of the coefficient is of course an equipment of the observer, not of the agent observed.

It is no part of our undertaking, in seeking an expression for *intelligence*, to find interpretation in some traditional category for every auxiliary expression that may be picked up along the way; nevertheless, it may be useful on some other occasion to consider whether the coefficient just introduced might not serve to measure the attribute called *intellect*. To make it so serve, the simplest device would be to confer on a constant of proportionality such dimensions as would make the whole expression, *constant* × *coefficient*, interpretable as a measure of *intellectual level*. This definition of *intellect* implies an agent on a *higher* intellectual level to be in possession of a better equipment of some sort; it does not imply this equipment to have been come by in any particular way. Such "intellectuality" may then have become

the subject's possession without the exercise on his part of any such "intelligence" as would promise an "intelligent expansion" of it; and it would be quite consistent with this interpretation of the *intellectual* to observe that a "highly intellectual man" may yet remain an "unintelligent fool". It would be natural too for one who interpreted the coefficient of rationality as a measure of *intellectual height*, to suggest its correlative, the coefficient of irrationality, as a measure of *instinctive depth*; and such an interpretation would obviously be consistent with that historic use which has opposed to *instinct*, not *intelligence*, but *intellect*. It is easy to see however that such intellectual height and such instinctive depth, reciprocally related as they are, would be adjectives primarily of an agent's *types of activity*, varying from type to type; while the "ideal category" we here aim to define under the name of *intelligence*, is to be an attribute of the *agent-as-a-whole*, invariant in all his activities. We need then pursue no further the definition of intellect; its discussion may have cleared the way for the next step in a definition of intelligence. This is a step which makes use of, but does not content itself with, the coefficient just defined, apt perhaps to measure intellect but inadequate to a measure of intelligence.

Whatever this coefficient itself may measure, the preceding reflections on the history of *intelligence* and *instinct* will sufficiently explain why we should now look beyond any value the coefficient may take on, to a rate of change of this value *with* something. And if this rate-of-change is ultimately to connect itself with the historic and still current idea that intelligence is somehow to be measured by the rate at which one learns to meet and master new things, then the *something* with which, in any intelligent process of learning, one's "intellectual equipment" varies can only be characterized as *experience*. That is, "learning" is conceived to be "an increase of rationality with experience". "Experience" for the moment may be left an entirely vague term; it is evidently not to be identified off-hand with any and every kind of lengthening history. Yet in any ultimate definition, *experience* must, along with *history*, be accorded "length of duration"; and its intervals must be made susceptible of various "fillings". So that, "increase of rationality with experience" is a phrase definitely

suggestive of a *ratio* in the form of a differential coefficient; and *experience-rate-of-change-of-rationality* is a concept of the the same mathematical order as (say) *time-rate-of-change-of-position*. If "experience", in the sense in which we commonly say "wisdom grows with experience," can be defined as a measurable quantity, the ratio, *coefficient of rationality ÷ experience*, is a measurable rate-of-variation. It seems to be both logically permissible and consistent with all that history has meant to connote by the terms involved, to define as follows:

$$\text{Degree of Intelligence} = \text{a direct function of } \frac{\text{coefficient of rationality}}{\text{experience}}$$

$$\text{Degree of Instinctiveness} = \text{an inverse function of } \frac{\text{coefficient of rationality}}{\text{experience}}$$

The meaning of this definition may be sufficiently illustrated by formulating a few of its more obvious corollaries. If, in the performance of any one type of act, an organism's rationality can be observed to increase with its experience, we attribute to that organism a measure of intelligence. If of two organisms, the rate of increase of rationality with experience is different, the organism of the higher rate is the *more intelligent*, that of the lower remains *more instinctive*. Neither *intelligent* nor *instinctive* is properly the adjective of a given act; yet such colloquial uses as, "that was an intelligent thing to do"; "that was purely instinctive on his part", are easily to be accounted for, and shown to be harmless "foreshortenings" of speech. For in fact, every observation of an act performed by an agent in a given situation yields a new datum from which the agent's intelligence can be estimated; but the estimate is only made by considering whether, in view of this new evidence, the rate at which the subject is capable of elevating its "intellectual level" with increasing experience is greater or less than previously estimated. The expression "an instinctive act" is readily interpreted in an analogous (but inverse) way;

and it is to be noted that however the admirer of the instinctive may wonder at the complex adaptations of what he calls "an instinctive act," there is an implication of unintelligence in his very wonder. That an intelligent performer should have contrived these complex adaptations would be no marvel; the marvel is that they should be successfully performed *in season* by one so likely to perform them *out of season*, so unlikely to substitute for them others, of but slightly modified detail, which would have converted failure into success.

At this point, and in connection with the definitions just offered, it would be interesting to review the observations and interpretations of those comparative psychologists and animal behaviorists who have most carefully and experimentally dealt with such problems as "the origin of instinct", and "the birth of intelligence".[28] As all the data required for the measure of intelligence at any level of life are, to our understanding of this measure, of an obviously "objective" and experimental order, it is only by such work as these experimental psychologists have performed that facts may be collected on which our calculus could work. But first of all, it is only by gathering data of a kind to which may be applied, one after another, tentatively assumed forms of function, that we can determine more definitely the mathematical meaning of that "direct function of the rate-of-change of intellectual level with experience" to which "degree of intelligence" now stands equated. We have gone far in proposing this much; but are still only at the beginning of the problem of finding a "measure of intelligence," so long as we shall have found no way of giving full determination to the *form* in which degree of intelligence is a direct function of a certain empirical ratio. This determination of a form-of-function can be effected in none but the idiom invented to deal with problems of just this order, the idiom of that branch of mathematics commonly called "functional equations". To apply the methods of this mathematical discipline to a problem whose data are empirical, whose determining conditions are vague, is a task of no little difficulty. There is but one place in which one may hope to collect empirical data sufficiently controlled as to their nature, sufficiently close-read as to

their values, to serve as test-material in this connection; that place is the psychological laboratory. There is but one class of beings articulate enough as to their meanings, well-formulated or implied, to suggest any of those determining conditions without which the best technique of functional equations is left with nothing to work on. For these reasons, the remainder of the present study will devote itself to a single problem, narrow and illustrative rather than inclusive and conclusive, but at least experimentally and mathematically "manageable". It is the problem of designing laboratory tests, such that the experimental returns may be used as data for calculating the subject's *degree-of-intelligence*, on the basis of a definite mathematical formula. The formula itself can only be derived by subjecting our first indeterminate expression "intelligence = a direct function of rate of change of rationality with experience" to certain formal demands of a mathematical order. The demands themselves however are merely the expression, in a mathematical idiom that makes them available for the purpose in hand, of the demands history and current use put upon the term *intelligence*, as a word of common discourse. It is to be presumed we shall be driven to find special terms to express the exact kind of "rationality"and "experience" that can be made available for laboratory purposes; and it is not to be presumed that we shall be able to analyze out of the complex factors making for any "unintelligence" the laboratory may expose, all the various instinctive factors that are its most likely ingredients. It is obviously only at the price of leaving the higher generalities to be discussed later, that a science in the making can hope to reduce a problem, only vaguely known to be "of an experimental order", to a question well enough formulated to admit of experimental answer.

The remainder of the present study will be occupied with the design and conduct of experiments testing out the *form* to be given the still indeterminate *function* entering into our preliminary definition of intelligence.

IV

Intelligence as a Measurable Function of "Efficiency" and "Experience"

Intelligence having reached a first expression as "a direct function of the ratio, *rationality ÷ experience*", any further determination of the *form* this function should take on (to satisfy the demands reflection would impose on a definition of this category) must require the aid of laboratory methods. But before this aid can be given, it will be necessary to reduce all matters for measurement involved to proportions permitting them to be brought within the confines of a laboratory. The quantities without whose exact measure we could proceed no further toward observing how they vary, one with respect to the other, are (1) a certain numerator, *coefficient of rationality*, and (2) a certain denominator, *experience*. Let us examine these in turn. The *coefficient of rationality*, itself a ratio, has already been defined in a way that makes the finding of a numerical expression for it a problem of "an experimental order"; but the observation involved in any relevant experiment is of an order too vast to be brought within manageable limits. If we are to set down experimental values for a certain ratio (analogous to *coefficient of rationality*), with a view to studying the way these values vary with a certain other measurable quantity (analogous to *experience*), we must seek among the adjectives applied to human performances that one which in all the relations significant to our problem is most of a kind with the "relative rationality" of biological performance throughout nature. As such an analogue of the coefficient of rationality entering into our first expression for intelligence, we propose a certain *coefficient of efficiency* immediately to be defined. When this definition shall have been offered, it will be seen that the *efficiency* therein defined is an exactly measurable attribute of human performance; that its measure is determined by the observation of certain "right performances" (r), and "wrong performances" (w), such that throughout the later discussion these quantities can be made to stand in much the same mathematical

relations to all other terms involved, as do the "rational performances" (r) and the "irrational performances" (i) of our original expression for *coefficient of rationality*. Now, as it is an experimentally manageable problem to study the way in which a certain *coefficient of efficiency* (into which enters the ratio $\frac{r}{r + w}$) varies with a certain quantity called *experience*, we risk the hypothesis that any mathematical function found to express the law of this variation cannot differ greatly in *form* from the mathematical function which we should find to express the variation of a certain *coefficient of rationality* (into which enters analogously the ratio $\frac{r}{r + i}$) with a quantity likewise to be called *experience*. Only at the price of such risks can a science in the making hope to bring problems said to be of an experimental order, to the proportions of test questions seen to be experimentally answerable.

The other term, *experience*, entering into our original expression for *intelligence*, is again a quantity whose measure is generally taken to present a problem of experimental order. "How much experience have you had?" is considered by employers to be a perfectly practical question. But it will readily be admitted that to determine *how much* significant experience an agent brings to bear on the performance of a given moment of that agent's history, is with difficulty to be brought within the reach of exact experimental measurement. It is however possible and even easy to record the number of trials to which a subject has been subjected prior to some given trial. We propose then to study the way in which "efficiency" (as it is about to be defined) varies with a quantity we shall *call* "experience"; a quantity whose numerical value will be known when a certain "number of previous experiences" is known. In doing so, we risk a second hypothesis; we suppose that we can give to our "measured experience" of the laboratory test such meaning as will justify the conclusion we would draw from our study; namely, that any mathematical form found to express the law of variation of a cer-

tain ratio with the measure of our laboratory "experience", cannot differ greatly in *form* from the function we should find to express the variation of a certain "analogous ratio" with the *experience* entering into our original expression for *intelligence.* The necessity and scientific value of such risks has already been commented on.

We turn then to such formal defining and such experimental observing as will enable us to determine whether there is any function of mathematical form, such that an *intelligence* said to be *this* function of *efficiency* and *experience* will conform, at once to the traditional demands laid upon the historic category of "intelligence", and to the methodological demands laid (in Chapter II) upon a certain "ideal category, *X*," demanded by the program of a self-perfecting psychology.

"Efficiency" in terms of "performance". Without pretending the following to be the only formula available, or even the one best adapted to the purpose of giving empirical measure to the *efficiency* of actual performance, we may at least define a *certain* "coefficient of efficiency" by giving mathematical expression to *certain* common notions on the subject.

In the performance of some task, let

r = number of *right* elements-of-accomplishment,

w = number of *wrong* elements-of-accomplishment,

n = r + w = *total* number of elements-of-accomplishment.

Then, $\frac{r}{n}$ will correspond to our common notion of "relative success of accomplishment"; $\frac{n}{t}$, to our common notion of "speed of performance".

"Efficiency" is commonly taken to vary in some sense with "relative success" and with "speed". Letting *C* stand for the *coefficient of efficiency*, this assumption may be expressed in the idiom of mathematics as the equation,

$$C = f\left(\frac{r}{n}, \frac{n}{t}\right) \qquad (a)$$

If now, we would determine the form of this function (f), we can have no guide but a series of "common notions" respecting "efficiency". These however may be given such mathematical expression as will make them available to the mathematician, as conditions determining the form to be given the unknown function (f). They may be conveniently expressed in the following postulates:

Post. 1. When all parameters are constant, efficiency (C) is a single-valued function of r, n, t.

Post. 2. For $\frac{n}{t}$ = constant,

(a) $\frac{r}{n}$ is a single-valued function of C;

(b) C is direct function of $\frac{r}{n}$;

(c) as $\frac{r}{n} \doteq 0$, $C \doteq 0$.

To conform with *Post. 1*, and the first two conditions of *Post 2*, the function sought must be one of the following forms:

$$C = A\frac{r}{n} + B; \quad C = A \log\left(\frac{r}{n} + B\right); \quad C = A\epsilon^{\frac{r}{n}} + B$$

The third condition (c) limits our choice to the form,

$$C = A\frac{r}{n}, \qquad (\beta)$$

where, recalling (α), we see A to be a function solely of the variable $\frac{n}{t}$.

To determine the nature of this coefficient, A, we derive from common notions a final postulate,

Post. 3. For $\frac{r}{n} = 1$ (the case of "complete success").

$$(a) \text{ as } \frac{n}{t} \doteq \infty, \; C \doteq 1$$

$$(b) \text{ as } \frac{n}{t} \doteq 0, \; C \doteq 0$$

If C is to conform to these conditions, A must be expressed as

$$A = \epsilon^{-K\frac{t}{n}} \quad ;$$

and we have for the final definition of *efficiency* (C) the equation

$$C = \frac{r}{n} \cdot \epsilon^{-K\frac{t}{n}} \qquad (i)$$

Here, K is a constant of proportionality, of dimensions $[NT^{-1}]$; it is numerically determined when "unit speed" is chosen. As this [N] is not merely an abstract number, but a number of "strokes", its numerical value depends upon what has been chosen to count as a (right or wrong) "stroke". Thus, in writing, either one *letter* or one *word* may be counted one *stroke;* but whichever choice is made must be adhered to wherever comparison of efficiencies is in question. Again, the speed $\frac{n}{t}$ may be expressed as strokes *per second*, or as strokes *per minute*, etc.; but the choice once made must be adhered to wherever a comparison is to be effected. K then is the parameter whose "constancy" is made a condition without which Postulates 1–3 would not correspond to our "common notions" of efficiency.

"Efficiency" in terms of "experience" and "intelligence". Again, our "common notions" on the subject are the only guide to an accurate, though tentative, expression for the way in which *efficiency* (C) varies with *experience* (E) and *intelligence* (I). We may in a first attempt to formulate this dependence of *efficiency* upon the *experience*, and upon the *intelligence* of the subject whose per-

formance is in question, confine ourselves to the simplest commonplaces; such as, "we learn by experience"; "practice makes perfect"; "the greater the intelligence, the greater the profit from experience". So that, having arrived at an expression for efficiency, whose value for a given performance may be determined experimentally, we may now put the meaning of our commonplaces into the idiom of mathematics; and by convenient postulates seek to determine the mathematical form of the function, f, in the expression,

$$C = f(E, I)$$

Post. 1. When all parameters are constant, each of the three variables, C, E, I, is a single-valued function of each other.
Post. 2. C is a direct function of E; and a direct function of I.
Post. 3. For I = constant,
(a) as $E \doteq 0$, $C \doteq 0$;
(b) as $E \doteq \infty$, $C \doteq 1$.

Post. 4. For E = constant,
(a) as $I \doteq 0$, $C \doteq 0$;
(b) as $I \doteq \infty$, $C \doteq 1$.

It will readily be seen that these postulates are necessary and sufficient to determine as the mathematical form of the function, f, the following:

$$C = 1 - \epsilon^{-KEI} \qquad \text{(ii)}$$

Here K is a constant of proportionality, of dimensions $[E^{-1} I^{-1}]$; whose numerical value is determined when *unit experience* and *unit intelligence* are chosen. These units may always be chosen subject to the conditions that for EI = 1, $C = \frac{\epsilon - 1}{\epsilon}$. Under these conditions, K is analytically unity, and may be dropped from further discussion, which will assume the final choice of E and unit I to be made consistently with the conditions named.

"Intelligence" in terms of "experience" and "efficiency". From (ii), we have at once

$$I = -\frac{1}{E}\log_{\epsilon}(1 - C) \qquad \text{(iii)}$$

Of the quantities entering into this final expression for *intelligence*, only one has in the previous discussion been shown to be within the reach of experimental determination; namely, the *efficiency* (C). Any "performance" whose "relative success" and whose "speed" can be conveniently observed will serve to furnish "efficiency values". *Experience*, on the other hand, opposes every obstacle to one who would subject it to measure. It obviously depends on a factor varying with the *previous history* of the performer; a factor of greatest importance to the result, yet generally beyond the observer's knowledge or control. It is necessary therefore in devising any *measure* of intelligence so to arrange the experiment that the data it yields will enable the experimenter to deduce from their internal evidence the part played by past experience in the efficiency of the present performance. To meet this condition, we suggest the following general design for all intelligence tests.

Design of "intelligence tests". Select a number of "tasks" such that an efficiency, C, for each performance may be calculated by formula (i); and let the tasks be as "new" as possible to the experience of all subjects compared. (This latter condition is not essential to a definition of the concepts involved, but convenient for historical comparison.) Repeat each task n times, keeping the length and "filling" of all intervals between tasks as nearly constant as possible. At the beginning of the nth task, the total "previous experience" (E_n) of the subject may be regarded as the sum of certain constituent experiences, $e_1, e_2 \ldots e_n$. The whole sense of this reconstruction of a total experience out of the successive increments of which it is the sum, is to be caught from the equation,

$$E_n = e_1 + e_2 + \ldots\ldots + e_n; \qquad \text{(iv)}$$

where e_1 is that unknown element, the subject's whole "life experience" prior to some "first test"; e_2 represents an "increment of experience" contributed by the first test plus the life

history filling the interval between the first test and the second; $e_3 \ . \ . \ . \ e_n$ represent analogous increments contributed by such subsequent tests and intervals as are indicated by the subscripts.

Experimental series to determine I for one subject. Attaching appropriate meanings to symbols introduced in (iii) and (iv), a single "task" repeatedly set a single subject under conditions of the previous paragraph would yield results to be expressed as follows:

$$1.\ I = -\frac{1}{e_1}\log\,(1 - C_1),$$

$$2.\ I = -\frac{1}{e_1 + e_2}\log\,(1 - C_2),$$

$$\ldots\ldots\ldots\ldots \qquad (v)$$

$$n.\ I = -\frac{1}{e_1 + e_2 + \ldots + e_n}\log\,(1 - C_n);$$

where the *efficiencies* $C_1, C_2 \ . \ . \ . \ C_n$ are experimentally determined by tests 1, 2 . . n; and the sum, $e_1 + e_2 + \ . \ . \ + e_n$, integrates all experiences acquired prior to nth test.

To have made the left hand members of the n equations identical, is equivalent to having accepted a new postulate in addition to the four already set down; namely,

Post. 5. For a single subject repeatedly performing an identical task, the *intelligence*, I, is constant.

In this postulate and throughout the present study, to speak of intelligence as a personal constant assumes merely that in the life span of an individual subject there is a finite period of time within which the intelligence is constant. The postulate makes no assumption as to the way in which intelligence may vary from period to period throughout the life history of an individual subject. The variation of intelligence with age is left a purely experimental problem.

From any two of the n equations of (v), say equations j and k, we may eliminate the term, E_j, representing the total experience prior to the earlier test, j. This prior experience will be represented by the summand common to the two denominators; and may be made explicit in the equation

$$E_j = e_1 + \ldots + e_j = -(e_{j+1} + \ldots + e_k) \frac{\log(1 - C_j)}{\log(1 - C_j) - \log(1 - C_k)}$$

Whence, substituting in the kth equation of (v), we have,

$$I = \frac{1}{e_{j+1} + \ldots + e_k} \log \frac{1 - C_j}{1 - C_k} \quad \text{(vi)}$$

As a special case, let j and k be equations 1 and 2 respectively. An expression for intelligence, I, independent of the whole "life experience" of the subject prior to the first test (*i.e.* the element of experience e_1), is obtained by eliminating e_1 from equations 1 and 2, giving the resultant expression,

$$I = \frac{1}{e_2} \log \frac{1 - C_1}{1 - C_2};$$

where, it will be remembered, e_2 represents the increment of experience contributed by that bit of the "life history" of the subject composed of the first trial *plus* the "filling" of the interval between the first and second trial. This bit of history is under observation and (so far as circumstances external to the subject are concerned) under control. The experience prior to the beginning of the test series has disappeared, as a matter of experimental interest.

Although the expression (vi) eliminates this whole past experience from the matters of experimental concern, one attempting to construct a method of measuring intelligence is as yet far from having attained his objective. The filling of the intervals that contribute the increments e_2, e_3 etc., is of course observable, and may be kept approximately constant throughout a repetition of an identical test on a single subject. But none of these quantities is measurable as an increment of *experience*; they are only comparable as equal and like-filled *intervals of history*. We may not assume, even when dealing with a single subject and the repetition of an identical test, that the increments e_2, e_3 etc., are equal in "teaching value"; we cannot even assume that they will all mark "advance", *i.e.* remain of *positive sign.*

Now, that every *increment of experience*, e_k, should be *positive* (necessary if I is to be *positive*) requires that for all the experi-

mentally determined *efficiencies*, C_2, C_3 etc., C_k shall be $> C_{k-1}$. And that all values of e_k should be *equal* (necessary if I is to be *constant*) requires that the experimental values, $\frac{1 - C_{k-1}}{1 - C_k}$, shall remain constant for all values of k. But no one accustomed to the great irregularity of experimental results gathered from the "responses" of living subjects, would expect the returns of any experiment testing the "growth of efficiency with practice" to fulfil such mathematical requirements as these. Yet, unless we can utilize such experimental data as one may expect to gather from a "practice experiment" of some sort—utilize them to justify us in attributing a "constant intelligence" to a single subject, at least under favorable conditions—there is no hope of measuring the intelligence of an individual nor of comparing the intelligences of two individuals by use of the simple formula just suggested.

Before we can estimate the difficulty of the task here involved, before we can judge of the possibility of its solution, it is essential to set up a variety of experiments, and to deal with the actual returns these will have yielded.

V

Experimental Determinations of "Efficiency" and "Experience"

Of the several types of experiment to be described in the sequel, the one that proved least adapted to the practical "measurement of intelligences" is nevertheless chosen for first presentation, because of the ease with which the experimental problem of "measuring efficiencies" may be illustrated in terms of this procedure.

Typing Test. Using an Underwood in one case, and a Hammond in the other, the author gave to two subjects, in the way of a preliminary experiment, the "new" task of learning to type a given text-model. Through this preliminary experiment it was learned how important it was to give many and careful directions regarding the manipulation of the machine, and the doing of the work. It was found necessary to explain how to insert the paper; to shift the carriage; to use the space bar; to make capital letters; to use different fingers in striking the keys; etc. It was learned that a subject given this test must be warned in advance against any incorrect memorization of the task to be written (such as the substitution of "the" for "a"); against stopping to correct errors; against looking back over work already written; etc. It was learned that the examiner must watch the performance carefully for such accidents as skidding of the carriage, in case the question should later arise, whether in certain eventualities such accidents had occurred, or whether the space-bar had been struck more than once.

The "new" task of learning to type a given model was then given to a number of subjects, generally thought "intelligent" in the popular sense of the word. The model selected was the sentence, "A quick brown fox jumps over the lazy dog." This model was chosen because it gives in miniature the whole story of the learning process, for in it each letter of the alphabet occurs at least once; moreover, the words are short, easy to spell, and the letters better distributed over the keyboard than those com-

posing either of the other two models generally used for this kind of practice. The preliminary experiment had revealed the fact that it was advisable to give the trials on the same day; to let them consist of ten-minute periods of work, each followed by a ten-minute rest period; and to restrict the strain on effort and attention to the demands of no more than nine trials. The subjects were directed to write the sentence again and again as many times as possible during the interval allowed for work, so that each trial should yield a record in miniature of the learning process, and the nine trials together would establish a sufficient number of *efficiences* to test the availability of our formula (iii) to the experimental measure of intelligence.

Calculation of Efficiency. After the experiment was complete, the papers were scored in the following manner: *One stroke* was counted for every small letter and every period; *two strokes* for every capital (since a second movement had to be made with the shift-key in recording it); a *half-stroke* for striking the space-bar between words and sentences; *two strokes* for shifting the carriage at the end of a line. On this basis, n, the number of possible elements of accomplishment, and r, the number of units of right accomplishment were computed. The ratio $\frac{r}{n}$, appearing in our expression for *efficiency* (C), having been thus given a meaning for experiment, the remaining ratio, $\frac{n}{t}$, may be fixed by dividing the *number of strokes* ($n = r + w$) recorded in any one trial, by the *time* (t) in which they have been made. (So that, if *time* be expressed in *minutes*, the "unit-speed", K, of our formula, will be fixed at *one-stroke-per-minute*).*

*It may be convenient to have before the eye an actual computation of the *efficiency* determined by an experiment in which r = 86, n = 95, t = 10 minutes.

The expression for efficiency,

$$C = \frac{r}{n} \cdot e^{-\frac{t}{n}}$$

reduces to the logarithmic form

$$\log C = \log r - \log n - \frac{t}{n} \log e$$

We are now prepared to consider the experimental *efficiencies* established by applying the *typing test* to a single subject. The data obtained from each of five subjects will be found in Table I, (Appendix). The most casual inspection of these returns shows at once that the experimentally determined values of C, even for a single subject repeating an identical task, vary in a way that suggests the part played in the result by (1) accidental disturbances, whose affect on efficiency is now to increase, now to diminish it; (2) more or less regular rhythms, probably independent of accidental disturbances; (3) a more or less systematic diminution in rate-of-learning, probably due to fatigue. It would be a problem of indefinite complexity to remove, or allow for, the causes of these "variable" and "systematic" errors which mask the part played by "intelligence" in a display of progressive efficiency. Therefore the experimenter interested in the present problem is reduced to devising a method by which his experimental returns may to the maximum degree *control*, though they do not immediately *determine*, a single curve representing efficiency (C) as a function of experience (E) in a learning-process throughout which *intelligence* (I) is postulated constant.

There is one convenient and theoretically satisfactory way of forcing accidentally varying data of efficiency to control a single curve which, for an intelligence postulated constant, shall represent *efficiency* as the function of *experience* we are tentatively supposing it to be. The method involves a process familiar to

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Using common logarithms, the computation proceeds,

$\log r$	=	11.93450 − 10	
$\log n$	=	1.97772	
$\log \frac{r}{n}$	=		9.95678 − 10
$\frac{t}{n}$	=	.10526	
$\log \epsilon$	=	.43429	
$\frac{t}{n} \log \epsilon$	=		.04571
$\log C$	=		9.91107 − 10
C	=	.8148	

the mathematician, frequently called upon to "flatten" broken lines into curves of describable form. The procedure is easily grasped from the first application we shall be required to make of it.

In the following array of figures, the first row contains the values experimentally obtained for a certain subject *A*, subjected to the nine trials of the *typing-test* just described. The second row contains the *arithmetical means* of the two first-line numbers between (but below) which each mean is placed.* Of the subsequent rows, each is related to its predecessor as the second row to the first.

TYPING TEST

9541	.9760	.9813	.9558	.9820	.9794	.9882	.9832	.9841
	.9650	.9787	.9686	.9689	.9807	.9838	.9857	.9837
		.9719	.9737	.9688	.9748	.9823	.9848	.9847
			.9728	.9713	.9718	.9786	.9836	.9848
				.9721	.9716	.9752	.9811	.9842
					.9719	.9732	.9782	.9827
						.9726	.9758	.9805
							.9742	.9782

If, now, at points 1 2 . . . 9 arranged horizontally and separated by equal intervals, one erect ordinates proportionate to the efficiencies shown by the 1st, 2nd, . . . 9th numbers of the *first* row of this table; and if through these points one pass a curve, this curve might be called the *experimental curve* platting the observed variation of *efficiency* with *number of trials*. But, evidently, a curve drawn through the points whose appropriately placed ordinates are the calculated efficiency values given in the *second* row of arithmetical means, will be *controlled by*, though no longer determined by, the experimentally observed efficiency values recorded in the first row. Such a curve may be said to represent the experimental curve "flattened one degree." The process of flattening may continue until the only ordinates are the two "resultant efficiency-values" given in the last line; by this time the experimentally determined curve will have been flattened seven degrees, to what we shall call "the limit of flatten-

*The *geometrical mean* may replace this *arithmetical mean* with some theoretical advantage, and at little added cost of labor.

ing." At each step, and in the limiting case, the "flattened values" are so definitely controlled by the experimental values, that a change in any one of the latter would be reflected in a change in at least one of the former.

A use to which may be put the two "resultant efficiencies" obtained from an experimental curve in the way described readily suggests itself. On the plat already constructed (of a given experimental curve), at the points on the base corresponding to the numbers 4.5 and 5.5 respectively, erect the ordinates C_i (= .9742 and C_{ii} (= .9782), the two resultant efficiencies of the typing-test just recorded. Through these, pass a curve of the form defined by our equation (iii); the two conditioning equations being in the present case,

$$e_1 I = -\log_\epsilon (1 - .9742) = \log_\epsilon \frac{1}{.0258}$$

$$(e_1 + e_2) I = -\log_\epsilon (1 - .9782) = \log_\epsilon \frac{1}{.0218}$$

where e_1, represents A's experience prior to a first (hypothetical) trial that has shown an efficiency C_i (= .9742); and e_2 represents the experience-increment added to e_1 in the interval between the beginning of the first and the beginning of a second (hypothetical) trial, this second trial having shown an efficiency C_{ii} (= .9782). Solving, we obtain*

$$\begin{aligned} e_1 I &= 3.66 \\ e_2 I &= .17 \\ \hline (e_1 + e_2) I &= 3.83; \end{aligned}$$

where $e_1 I$, the abscissa whose ordinate is C_i, represents the profit to an intelligence of grade I, of a "total past-experience", e_1, measured from a certain *zero*. This experience has raised the

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* That is, by a convenient arrangement of the calculation,

$$e_1 I = \operatorname{colog}_\epsilon .0258 = \frac{\operatorname{colog}_{10} .0258}{\text{mod. common log.}} = \frac{1.58838}{.43429} = 3.66$$

$$(e_1 + e_2) I = \operatorname{colog}_\epsilon .0218 = \frac{1.66154}{.43429} = 3.83$$

$$e_2 I = \qquad\qquad .17$$

intelligent subject's efficiency from *zero* to .9742. To this initial efficiency, the contribution of the subject's total past-experience ,e_1, the experience-increment ,e_2, then adds its contribution, raising the subject's final efficiency to .9782.

To visualize the relation of this "experimentally controlled" efficiency curve to the purely "experimental" curve with which we started, we have only to construct, on the same plat that already holds the experimental curve and the two points whose respective ordinates are the "resultant efficiencies," C_i, C_{ii}, the "controlled efficiency curve" passing through these points. As the ordinate C_i is now known to correspond to the abscissa 3.66 of the "controlled" curve and the ordinate C_{ii} to the abscissa 3.83, it is easy to set down the abscissae of all the points 1, 2 . . . 9 of our plat. These abscissae will be, in order,

3.065, 3.235, ... 4.425

separated by the common interval .17. It is now a pure matter of calculation to determine the ordinate corresponding to each abscissa;* namely, the ordinates given in the central column of the tabulation here following. Of this tabulation the first and last columns will be recognized as the "observed efficiencies" of the trials to whose number their respective rows correspond, the columns headed *minus* and *plus* respectively will be seen to contain the values by which the observed efficiencies fall short of or exceed the corresponding "controlled" efficiencies.

C (experimental)	MINUS	C (controlled)	PLUS	C (experimental)
		.95333	.00077	.95410
		.96064	.01536	.97600
		.96681	.01449	.98130
.95580	.01621	.97201		
		.97634	.00566	.98200
.97940	.00062	.98002		
		.98317	.00503	.98820
.98320	.00261	.98581		
.98410	.00393	.98803		
Total.....	.02337		.04131	

* A convenient arrangement of the calculation, first ordinate, abscissa 3.065:

A further analysis of the relations found to exist between empirical curves and controlled curves, as determined by tests of the kind here described, might furnish some observations of interest. In particular, an important technical problem is raised by the question of our ability further to control and ultimately to correct such curves in a way that will have neutralized the systematic error of fatigue, and perhaps some of the "longer rhythms" one suspects to appear in a variation of efficiency with practice. The present study must confine itself to a preliminary survey of the extent to which such further analysis of details is called for by the general problem of measuring intelligence. It contents itself therefore with offering a graphic reproduction of the "controlled curve" of the subject A, which curves it has been thought more interesting to present in connection with the empirical curves (Plate 1). With these illustrative graphs, it would associate a note or two on the value to be accorded "controlled curves" as experimental data.

The results of having substituted "controlled" for "experimental" curves throughout the remainder of this study, seem to confirm the following assumptions:

1. The data furnished by a single subject, performing repeatedly an identical task, lie so distributed about a certain curve, controlled by, but not determined by the returns, as to encourage the assumption that if experience, intelligence, efficiency, were the only variables in an experiment of this kind, they would stand related to one another in the way expressed in our equation (iii). That is, the departures of the empirical data from the curve calculated under their control are such as may be attributed to unknown sources of variable error, and suspected

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modulus com. log = .43429
log modulus = 9.63778 − 10
log 3.065 = .48643

$\log \epsilon^{-e_1 I} = .12421 = \log 1.331 = 8.669 - 10 = \log .04667$

$1 - \epsilon^{-e_1 I} = .95333 = C_1$

sources of systematic error not yet subject to control.

2. The *form* of the curve is consistent with the idea that if these sources of error could be avoided, two independent variables, "experience" and "intelligence" would, if they could be separately controlled, be sufficient accurately to account for the generally assumed "increase of efficiency with experience," and to provide for an expected *inequality of increase*, by allowing the rate-of-increase to "depend upon the intelligence of the experiencer."

3. The technical position assumed is that, with a single subject repeatedly performing an identical task, the intervals of *history* equal in duration and like in episode which make up the history of the experiment, may be regarded as determining equal increments of a composite quantity, e I, whose dimensions are those of a product in which *experience* and *intelligence* are the only factors. It remains to be seen whether we may make the factors in this product vary independently one of the other in such wise as to let us study, first, the dependence of efficiency on experience, under constant conditions of intelligence; second, the dependence of efficiency on intelligence, under constant conditions of experience.

4. An hypothesis, whose mathematical meaning, at least, is clear, suggests that if we vary the type of task, keeping the subject the same, we may be able to vary, in products of the form eI, the factor e independently of the factor I; while if we vary the subject, keeping the task the same, we may be able to vary the factor I independently of the factor e. The only way in which this hypothesis may be subjected to experimental test is, of course, to devise a number of types of task, and to present all the varieties devised to a convenient variety of subjects. The next step then must be, to introduce new experimental procedures.

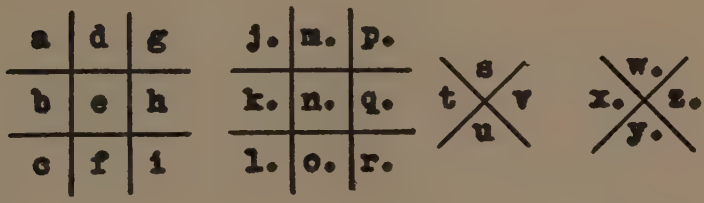
We may refer to the two tests about to be described as the "Number Code" experiment, and the "Binet Code" experiment, respectively. In both, the method of testing individual progress in learning to reproduce a given model in code, was kept as nearly as possible identical with that employed in testing an individual's progress in learning to type a given model. The sentence remained the same; the number of trials (nine) the same as before;

these were given on the same day, each work period being followed by a rest period of the same duration (ten minutes).

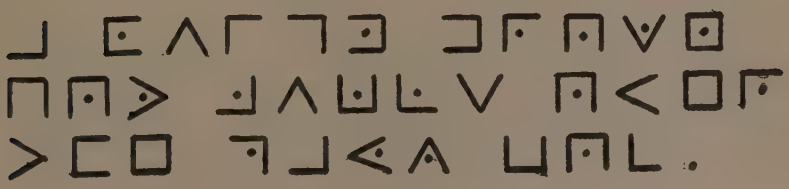
Number-code Test. The task required that in place of the letters of the given words, the subject write digits from one to twenty-six, of which each was to stand for the like-numbered letter of the alphabet. (1 for a; 2 for b; etc.) The sentence, *A quick brown fox jumps over the lazy dog*, would then read in code: 1—17 21 9 3 11 —2 18 15 23 14—6 15 24—10 21 13 1619—15 22 5 18—20 8 5— 12 1 26 25—4 15 7.

A subject's record of performance was then scored on the basis of the following estimate of "strokes"; namely, *one and one-half* for every number consisting of two digits; *one stroke* for change of lines; and *one-half stroke* for each space properly left between words and sentences.

Binet-code Test. Retaining the model-text, the subjects, the experimental procedure of the previous experiments, the task set in this third series was, to write the selected sentence in the "Binet" code, which is after this manner.



The former model sentence correctly written appears as in the following lines:



The results were scored thus: *one stroke* for *one-half* for · or ; *one and one-half* for ; *two strokes* for ; *two and one-half* for ; *one-half* for *period;* and *one* for *change-of-line.*

The coefficients of efficiency, C, computed from the results of these two experiments, appear in Table I, under "Number", and "Binet", respectively. The two "resultant efficiencies" obtained from the nine observed efficiencies by the flattening method (described p. 68) are given for each test and each subject in Table II; together with the values of eI calculated from these by their substitution in equation (iii) or (vi)*.

It will be convenient to consider in order the data furnished by an application, first, of a variety of tests to a single subject; second, of one type of test to a variety of subjects; third, the entire variety of tests to the entire variety of subjects.

One Subject, Variety of Tests. Continuing with the subject A, whose record served to illustrate the Typing-Test, from Table II we reproduce for immediate inspection the records

	NUMBER		BINET		TYPING	
	C_i	C_{ii}	C_i	C_{ii}	C_i	C_{ii}
Resultant C_i C_{ii}	.9457	.9643	.9629	.9699	.9742	.9782
Calculated eI	.42		.21		.17	

As there can have been no reason to expect that even the most constant intelligence would derive the same profit from intervals of history, equal as to their duration, but different as to their episodes, it cannot surprise one now to find no constancy in the values of eI obtained, for the same individual indeed, but for that individual subjected to a variety of tests. The only question of interest concerns the manner in which we should interpret the variation of these values for a single subject. Here, our postulate

*cf. p. 61; and p. 63.

must be the analogue of that which guided the interpretation of the variation of efficiency in *one* individual subjected to *one* type of test. The postulate there accepted was the one most consistent with the concept we are testing; namely, that the "intelligence" (I) defined in equation (iii) is invariant for the same individual however his efficiency, (C), vary with practice (p. 62). The postulate resolving the present question of interpretation must then be

Post 6. For a single subject, the *intelligence*, I, exhibited in any two types of performance is constant.

There is of course no theoretical difficulty involved in conforming our interpretation of the varying eI-values of a single subject to this demand. Obviously, if eI is observed to vary, and I is postulated *not* to vary, the total variation of the product must be absorbed by its free factor, e. Now, not only has the previous discussion left the increment-of-experience, e, free to undergo just this variation, but our defining has awaited the moment for exercising this freedom, to exhibit more fully the meaning it attaches to *experience* in general, *increment-of-experience* in particular. It has not been assumed that experience was a variable whose "growth" was measurable by laboratory devices at the beginning of our experiment, leaving the only experimental problem before us a gathering of data enabling us to determine the *law* by which a measurable efficiency varied with an equally measurable experience. The order in which our thought has proceeded will seem for a moment like a "circle in defining," but must appeal in the end as a sound method of defining for experimental use. We may put our whole thought on the subject of developing definitions into a single application of it to the case in hand. At the outset, we learned how to measure *efficiency* as objectively as we already knew how to count numbers and to measure time. We did not and do not yet know how to measure *experience* or *intelligence;* still less do we know how the three quantities, *efficiency*, *experience*, *intelligence* vary one with the other; there are common notions on the subject, nothing more. The procedure which we pretend to be the only one open to the scientific defining of terms and formulating of law is to progress step by step to a *simultaneous defining and formulating.*

But how may each of these many loose ends be kept loose until all are tied together? The answer lies in the use of postulates, which might be likened to a provisional tying of knots, later to be readjusted if advisable. One may recall our procedure, so far, in some such order as this. From the beginning, we could recognize difference in *types of history* (kinds of "test"); could *count the number* of like episodes in each type (number of "trials"); could *measure the time-intervals* between episodes. After some reflection, we found it possible to be just as objective in our *measure of efficiencies*. But in what followed on these beginnings, the procedure has been groping and tentative; definitions have been taken on trial; "postulates" have literally *asked* to be accepted "only for the sake of argument". What then is the argument? It is one whose steps are justified only if they lead to a conclusion; the validity of the steps depends on the end they lead to; till the argument shall have reached its end, its one guiding motive must be to discover how far it is possible to interpret the objective returns of its experiments consistently with any postulates it may be willing to formulate, guided by "common notions".

Now, it is consistent with all that has yet been postulated and defined, to accept an interdependent meaning for the categories *experience* and *intelligence*, the terms in which we must continue our discourse. *Intelligence* (we may say) is that which varies with *experience* according to *the law* expressed in our equations (iii) and (vi); *experience* is that with which *intelligence* so varies; *the law* is that which makes its variables to be *experience* and *intelligence*, rather than quantities of other name. There is then nothing in our experimental returns that prevents us from interpreting the intelligence of a single subject to be invariant through all variations of its expressions in performance. On the contrary, it could be quite possible to plat a consistent graph of all that has been so far assumed or discovered: in Plate III will be found a curve whose abscissae are values of *experience* × *intelligence* (eI), and whose ordinates are values of *efficiency* (C). The curve is drawn to the scale, unit-eI $= \frac{\epsilon - 1}{\epsilon} = .6321$; unit-C, any arbitrary distance along the C-axis at which we wish to

draw the horizontal that shall serve as asymptote to a certain curve; and of this distance, the C-readings are percentages. The curve itself is the graph on the scale just indicated of our function (iii), here conveniently conceived in the form,

$$eI = \log_{\epsilon} \frac{1}{1 - C}$$

Now, as in the discussion of the performance of a *single* individual nothing depends on the numerical value attached to the *constant* that, under *Postulate* 6, is to be assigned to his individual intelligence, we may conveniently assign to the Subject A of the experiment before us an intelligence equal to *unity*. In which case, the values e_1I, e_2I; C_i C_{ii}, calculated by this formula from the experimental results of the three tests above described, would take the positions indicated in the graph by an appropriate placing of the letters N, B, T (respectively, Number-Test, Binet-Test, Typing-Test).

As, obviously, analogous graphs could be constructed for all subjects, no matter what the experimental returns, it is impossible for experiment to develop any inconsistency between our definitions and postulates expressed in terms of *efficiency*, *experience*, *intelligence*, on the one hand, and on the other, the facts offered us for description in terms of these categories. The argument so far has met with no empirical obstacle calculated to make one withdraw one's provisional assent to the postulates asked for the sake of that argument's development. But this is not to say that when we pass from the consistency of our account of *one* "intelligence", recognizing a definite functional dependence of his efficiency on his experience, to a comparison of *several* "intelligences", we shall continue to find it possible to abide by postulates already formulated. To test this possibility, we take the next in order of the three steps announced in a previous paragraph; namely, the consideration of

One Type of Test, Variety of Subjects. The "controlled" values of eI obtained from each of our five subjects submitting himself to our three types of test, appear in the columns of Table II. For compactness, we reproduce here in row-form the first column of the table,

eI-VALUES

	A	B	C	D	E
Number Test...	.42	.49	.41	.31	.39

The only immediate interest in this step of our development, is the freedom we as yet enjoy to entertain any one of a number of postulates, no one of which could contradict our previous assumptions, any one of which would result in establishing a definite mathematical relation between the *intelligence-grades* of the five subjects. The simplest assumption, and therefore the one most commonly made in standard intelligence-tests, may be put in the form of the following

Traditional Postulate. For a single type of test, performed on more than one subject, the *experience-increment*, e, is constant.

This postulate is worded in closest possible analogy with the last, *Postulate 6*, in order to emphasize the mathematical sufficiency of the two postulates taken together to a comparison of any two "intelligences"; each graded consistently with *Postulate* 6, making individual intelligence constant; the two compared in the light of the *Traditional Postulate*, justifying the equation,

$$\frac{\text{Intelligence of A}}{\text{Intelligence of B}} = \frac{\text{eI of A}}{\text{eI of B}};$$

the eI values being obtained from the same type of test. (*E. g.* the Number-Test figures as above reproduced would yield the comparison,

$$\frac{\text{Intelligence of A}}{\text{Intelligence of B}} = \frac{42}{49};$$

or, B is 1.17 times as intelligent as A.)

It is clear then that no inconsistency can develop to discourage us from accepting the *Traditional Postulate* along with the six that have preceded it, so long as we are content to compare various "intelligences" in terms of some one type of test; and

to arrange the e-values of any one "intelligence" subjected to a variety of tests in an order prescribed by the postulated constancy of individual intelligence. This double comparison is so far from presenting any logical difficulty in execution, that it is easy to imagine all the experimental results involved accommodated to a single graph conforming to the conventions just stated; a graph composed of a *family* of curves, of whose members I is the distinguishing parameter, each member representing the law by which the efficiency of an individual intelligence varies with his experience. This law would of course retain the form of the function defined in (iii); and so far we should have found nothing to render us dissatisfied with any of our definitions or postulates. But it will be noticed that with the acceptance of the last ("traditional") postulate we have availed ourselves of our last "degree of freedom"; while the final comprehensive survey of all our data is not yet completed. One already suspects that the crucial test of our definition of *efficiency, experience, intelligence*, must come at the last moment; the moment at which we are left with no freedom of assumption to accommodate our interpretations to the actual facts of the case. This last step in testing the adequacy of our conceptions brings before us the results obtained from a

Variety of Tests, Variety of Subjects. Again we accept the compacter row-formation for the reproduction of the *columns* of Table II.

eI-VALUES

	A	B	C	D	E
Number.....	.42	.49	.41	.31	.39
Binet.....	.21	.22	.24	.06	.09
Typing.....	.17	.14	.15	.13	.06

The briefest inspection of these results will reveal the fact that if all the preceding postulates be allowed to stand, any ratio purporting to give an experimental answer to the familiar question, How much more intelligent is A than B?, must reflect some arbitrary choice of the type of test on whose returns the numerical

ratio is calculated. There are doubtless a number of ways in which to express the failure of any interpretation so far developed to satisfy our ideas respecting, and still more our ideals perfecting, the concept of *intelligence* as a function of the ratio *efficiency* ÷ *experience*. It may be doubted whether any of these ways could be made more telling than the one just selected. Here at any rate we have a criticism of such weight that one may wonder whether any further reflection on the problem before us can find a way of lightening the situation without throwing away all definitions so far offered; together, of course, with all postulates asked in terms of these definitions.

VI

Critique of the Formula Used; Generalized Problem of "Intelligence"

The first consideration that suggests itself in presence of any experimental results gathered for the sake of determining whether a proposed law of nature "fits the facts", is the familiar one: "Is the probable error of the results low enough to enable one to determine whether the law proposed by a theory is excluded by the facts; or is the error so large that the law is left an entirely unconfirmed, but still permissible hypothesis?"

An answer to this question is difficult to give; at least in those numerical terms without whose use one answer is little better than another. The best that suggests itself in the way of forming some idea of the magnitude of the probable error with which we are working, would start with selecting some empirically given intelligence to serve as unit-intelligence. The only *unit-intelligence* for which there is any warrant in tradition, is that referred to in the popular comparison of a given intelligence with the "intelligence of the average man". To construct from our limited experiments the closest approximation we can hope for to the mentality of this "average man", we begin by taking the average eI for each of the three types of test, and for all five subjects, with the following results:

	NUMBER	BINET	TYPING
Average eI.....	.40	.16	.13

Next, we tabulate the ratio in which each individual eI of a given test stands with the average eI for that type of test, as follows:

RATIO

Individual eI ÷ Average eI

	NUMBER	BINET	TYPING	AVERAGE RATIO ± P. ERROR
Average Man.....	1.00	1.00	1.00	
Subject A.....	1.04	1.28	1.29	1.20 ± .11
Subject B.....	1.21	1.34	1.06	1.20 ± .10
Subject C.....	1.01	1.46	1.14	1.20 ± .17
Subject D.....	.74	.37	.99	.73 ± .21
Subject E.....	.96	.61	.45	.67 ± .19

In other words, the variation of the estimated intelligence of a single individual, A, estimate based on different types of test, is greater than the variation of the estimated intelligence of three three individuals, A, B, C, estimate based on same type of test.

Now it is true, as has been remarked, that while our method of "control" renders the controlled values of eI on which our calculations are based fairly independent of the more *accidental fluctuations* of efficiency in each test-series (nine tests), the "resultant Cs" remain affected in an important way by what seem to be *longer rhythms* of fluctuation, and by what seem to be *systematic errors* suggestive of fatigue. It is also true that further analysis of the results of single test-series might suggest a number of ways in which these factors, disturbing to any estimate of the part played by a "pure intelligence factor" in a test-series performance, might be analyzed out of the resultant effect. But awaiting further light on the eI-values that might result were these disturbing factors brought under better control, there is, on the strength of our present knowledge, but one safe judgment to be formed: Nothing justifies us in supposing that future control over conditions now disturbing experiment will bring us nearer-to, rather than further-from, evidence supporting the *Traditional Postulate.*

A second suggestion would recall that some have defined *intelligence* as dependent on, if not uniquely a function of, "mental balance". For any who would develop this suggestion, we have

at least perfected a method of determining mental-balance in a particularly accurate and convincing way; "mental balance" might be defined as a quantity inversely proportional to "probable error", as "probable error" (p) is calculated in the last column of the immediately preceding tabulation; that is,

$$\text{Mental balance} = \text{Const.} \times \frac{1}{\text{p}} .$$

Nor would there be any technical difficulty in expressing a final definition of *intelligence* conceived as a quantity directly proportional (say) to *mental balance;* for one need only retain the postulates from which the *intelligence* (I) of the preceding discussion has taken its meaning, using them now to define a nameless quantity, X, (which one might want to call "competency," or what not), then identify the *probable error* (p) of the last equation with the *probable error*, p (X), attached to this X at the end of a series of tests (multiple test, single subject). This definition of *intelligence* would then take the form

$$\text{Intelligence} = \text{Const.} \times \text{Mental balance} = \text{const.} \frac{1}{\text{p (X)}} .$$

Probably most would feel this suggestion to be worth further consideration only if one could start with the assumption that the *higher mean-X* would always be found associated with the *lower p (X)*. It happens that this assumption would be consistent with the experimental returns of the limited series of tests here examined, as a glance at the table (p. 82) will show. But the assurance is of the slightest that an indefinitely extended series would continue to confirm this assumption. What then would one decide, in case one met a *low mean-X* associated with a *low p (X)*, *i.e.* a *low competency* associated with a *high mental balance*? Whatever one conclude to be the basis of decision in such not only possible but fairly likely contingencies, must be put into the definition itself. *Intelligence* must become a composite of *mean-X* and *p(X)*; and no definition of *intelligence* will have been offered until the mathematical form to be given this function of two variables shall have been decided upon. This decision does not appear one to be quickly arrived at.

The last suggestion introduces a very broad subject. All formulæ into which *intelligence* can enter in any way, may be regarded as equations expressing *efficiency* as a function of *experience.* The formula we have been testing, introduces *intelligence* as the only named parameter in such a formula; the only quantity, other than *experience* and *efficiency*, which the formula permits an experimenter to measure. Other parameters are assumed constant, or their variations are made to "neutralize" each other in some form of "controlled" result. The suggestion that a function of the kind sought might include a *second* explicit and measurable parameter, *e.g.*, *mental balance*, raises the whole question of the range of possible parameters that might be used in coping with the returns of experiment. This seems to us the most general form in which can be expressed the question left open after the foregoing examination of the adequacy of the best function we could form with the use of a single parameter, *intelligence.* Using this parameter alone, our returns do not encourage one to think that an expression for *efficiency = function of experience* can be constructed with the use of *only one* namable and measurable parameter, whether *intelligence* or some other category (*e.g. balance*) be chosen for trial.

No discussion of *intelligence*, its definition, measure, relation to other categories of psychology, can be complete, that has not found the most general possible expression for this problem of the *multiple parameters;* that is, the problem of the array of parameters the experimentalist may be forced and must be prepared to use in a well-defined way, in establishing *efficiency* as a mathematical function of *experience.* But by way of guiding one's imagination to the final undertaking of this all-inclusive task, it will be helpful first to try-out a certain formula of *two parameters* only; the two parameters chosen having the closest relation to the two categories in which history has most frequently discussed the problem of this study.

The algebraic form of our expression (iii) is that of the special linear equation

$$c = a \cdot x \quad (1)$$

where c replaces our experimental constant determined by *efficiency* data (C); a replaces the *intelligence* factor (I); and x

the *experience* factor (E). A first step in generalizing the algebraic form of the function relating *efficiency* and *experience* would pass from this special to the general linear equation

$$c = a_0 + a_1 x \qquad (2)$$

This, and every other algebraic form of greater generality than the one we have been using (1), has been excluded from our consideration by one or another of the "postulates" formulated for the precise purpose of reducing, in conformity with their demands, the generality of the function-forms relating *efficiency* with *experience*. We have no difficulty in identifying the postulate which excludes this first suggestion of a more generalized function: it is *Post. 3*, (a), "For a given *intelligence* as *experience* approaches *zero*, *efficiency* must approach *zero*." What would it cost us to abandon this postulate? It will be remembered that all these postulates pretended to be guided by implications drawn from our "common notions" as to the meaning of the terms involved. It must be confessed, however, that some of these "notions" seem not very deep-rooted in human understanding; they were accepted, where accepted, as postulates which would bring the mathematician to the simplest formula *not inconsistent with* common notions. In the present instance, one feels that a proposal to sacrifice the demand made in *Postulate 3*, (a), so far from offending commonsense would be heartily accepted by so much of common sense as has found its way into history. For consider the interpretation to be put upon the term a_0, whose empirical meaning is given when the x of (2) is made *zero;* in which case,

$$c = a_0$$

This equation implies, of course, that a value greater than zero may be attributed to the efficiency of a subject, of intelligence however small, and of experience *nil*. Moreover, the term a_0 unlike the term $a_1 x$ which follows it, makes its contribution to *efficiency* once and for all; its offering does not increase with, is in no way modified by *experience*, whatever part experience may be shown to play in increasing the contribution made by the second term, $a_1 x$. If common sense, if all history were to be asked for a

name to attach to this algebraic quantity, a_0, (or to that whose measure is determined by this quantity), is there any doubt as to the answer it would give? Would it not with one accord suggest, "a is the measure of *instinct*"? Nor is there greater doubt as to what would be the common-sense answer to a question respecting the empirical interpretation of a_1. What is measured by a quantity a_1, such that if in two cases an "instinct" (a_0) and an "experience" (x) be of equal measure, that case to which is attributed the greater a_1 will exhibit the greater *efficiency*; and conversely? An a_1 so characterized can suggest but one name as appropriate to it: *intelligence*. Finally, here as always, one must ask in face of a proposed definition, By what experiment would one find the presence or absence of the thing defined, in a given domain of observation? In the present instance, the beginning of an answer is imposed by the mathematics of the situation. One must somehow find the c_1 c_2 x_1 x_2 of two situations justifying on empirical grounds the simultaneous setting down of two equations

$$\begin{aligned} c_1 &= a_0 + a_1 x_1 \\ c_2 &= a_0 + a_1 x_2 \end{aligned} \qquad (3)$$

It would be consistent with all experience demands, if we made definitional the empirically suggested assumption that in the same human being, repeatedly attempting to perform the same task, the contributions to that subject's efficiency-of-performance made by his *instinct* and his *intelligence* respectively would, for a test of sufficiently brief duration, remain constant. This is the justification required for giving, as in (3), identical coefficients to identical powers of x. The rest of the experimental use of this formula, the test of its adequacy to coping with all experimental returns, is the problem of a special study to be undertaken for formula (2), in a way analogous to that followed in the present study of formula (1).

And now, one may fairly well visualize the most general study that can be undertaken in the search for functions apt to express *efficiency* in terms of *experience*. The mathematician will at once propose as the final stage of any process of generalization, starting

as did this one with a transition from a special to a general linear form, a complete expansion of the function,

$$c = f(x) = a_0 + a_1x + a_2x^2 + \ldots \quad (4)$$

This higher generalization will, like the preceding, have been excluded by one of our original "simplifying" postulates; and again we have no difficulty in recognizing the postulate to be abandoned; namely, the clause of *Post. 1* which, for constant parameters, assumes *experience* to be a single-valued function of *efficiency*. This postulate, however, could readily be replaced by another, identifying *experience* with a certain positive real root of its expression as a function of efficiency. The simplifying assumption embodied in thus writing our general expression (the assumption that we may at least suppose the function we are in search of to be analytic), is consistent with all that experience can tell us at the outset of such a study; it may then be acted upon until the study itself shall have developed some incompetency, peculiar to analytic functions, to cope with experimental returns.

There appears at once the wisdom of so selecting the form of this empirically determined "experience" (x) as to make its numrical value fractional; the smaller the order of the fraction, the more rapidly do the terms in x of power higher than the first lose their numerical importance. The conditions of designing an experimental use of the completely generalized equation (4) are the same as those underlying the availability of the linear form (2); and, to begin with, we have always the reasonable assumption that for a single subject, single test, sufficiently limited time, we may write simultaneous equations with *some* identical coefficients for identical powers of x. From this starting point, the general study must proceed to a consideration of each successive experimental step, in a way analogous to that followed in the simpler study here undertaken.

The task is not necessarily endless; there is always the possibility that a finite series of experiments will justify the conclusion that all coefficients of terms of an order higher than some finite order disappear; that, consequently, there is a limited number of

coefficients for which the experimental psychologist need find empirical interpretation.

The conclusion of our own experimental study must remain, that in seeking an expression for *efficiency* as *function of experience*, to assume the disappearance of all coefficients of *experience* save that which may stand for *intelligence*, is too extreme a simplification of the general function to permit the resultant equation to give a self-consistent account of the materials furnished by experiment.

For the moment, we must content ourselves with having arrived at this well-tested result; and with having indicated the way in which the next step should be taken, in the study of *intelligence*, *instinct*, and such other categories of psychology as experience may demand for an experimental comparison of minds.

TABLE I

EXPERIMENTAL EFFICIENCY VALUES

SAME VARIETY OF TESTS GIVEN TO EACH OF FIVE SUBJECTS

SUBJECT A—COLLEGE SENIOR (FEMALE, AGE 21)

	1	2	3	4	5	6	7	8	9
Number..	.8749	.9073	.8654	.9670	.9523	.9818	.9787	.9852	.9833
Binet....	.8567	.9406	.9637	.9533	.9716	.9813	.9651	.9738	.9839
Typing...	.9541	.9760	.9813	.9558	.9820	.9794	.9882	.9832	.9841

SUBJECT B—COLLEGE SENIOR (FEMALE, AGE 21)

Number..	.7951	.8817	.8686	.9480	.9615	.9821	.9853	.9861	.9681
Binet....	.7619	.9345	.9315	.9273	.9621	.9597	.9671	.9740	.9718
Typing...	.9553	.9799	.9827	.9836	.9840	.9838	.9690	.9783	.9828

SUBJECT C—HIGH SCHOOL GRADUATE (MALE, AGE 32)

Number..	.8585	.8407	.9696	.9788	.9803	.9841	.9833	.9861	.9867
Binet....	.6744	.8838	.8900	.8760	.9274	.9352	.9639	.9529	.9351
Typing...	.9501	.9554	.9670	.9759	.9671	.9847	.9817	.9789	.9811

SUBJECT D—GRAMMAR SCHOOL GRADUATE (FEMALE, AGE 24)

Number..	.7428	.8084	.8783	.8928	.9194	.9547	.9566	.9747	.9798
Binet....	.7448	.7491	.8874	.8126	.7634	.8482	.8235	.8628	.8477
Typing...	.9241	.9428	.9606	.9694	.9667	.9781	.9695	.9795	.9833

SUBJECT E—JUNIOR HIGH, NINTH (MALE, AGE 16)

Number..	.6460	.9270	.9619	.9750	.9783	.9835	.9747	.9400	.9603
Binet....	.0504	.3017	.4199	.3546	.4123	.6264	.4822	.5273	.4012
Typing...	.9247	.9442	.9436	.9576	.9481	.9563	.9460	.9417	.9434

TABLE II

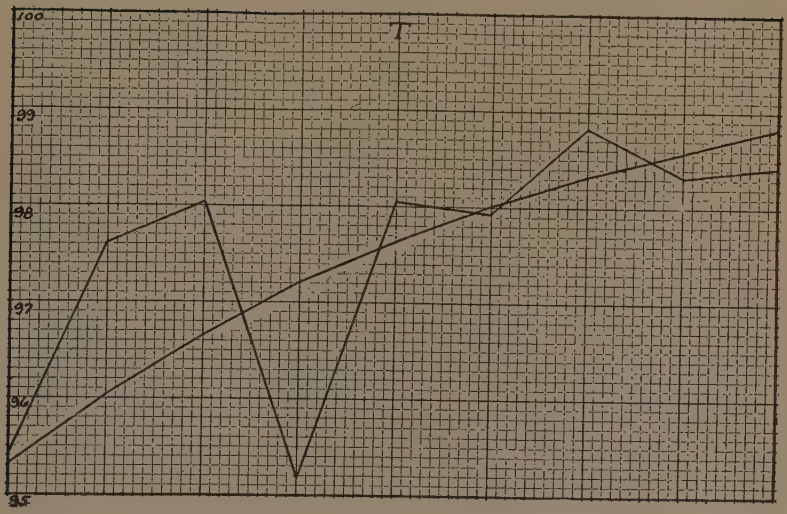
RESULTANT C_i AND C_{ii}, WITH CALCULATED eI

	NUMBER			BINET			TYPING		
	C_i	eI	C_{ii}	C_i	eI	C_{ii}	C_i	eI	C_{ii}
A	.9457	.42	.9643	.9629	.21	.9699	.9742	.17	.9782
B	.9420	.49	.9647	.9446	.22	.9556	.9824	.14	.9811
C	.9705	.41	.9805	.9065	.24	.9263	.9727	.15	.9766
D	.9063	.31	.9313	.8191	.06	.8292	.9670	.13	.9711
E	.9698	.39	.9752	.4290	.09	.4788	.9509	.06	.9510

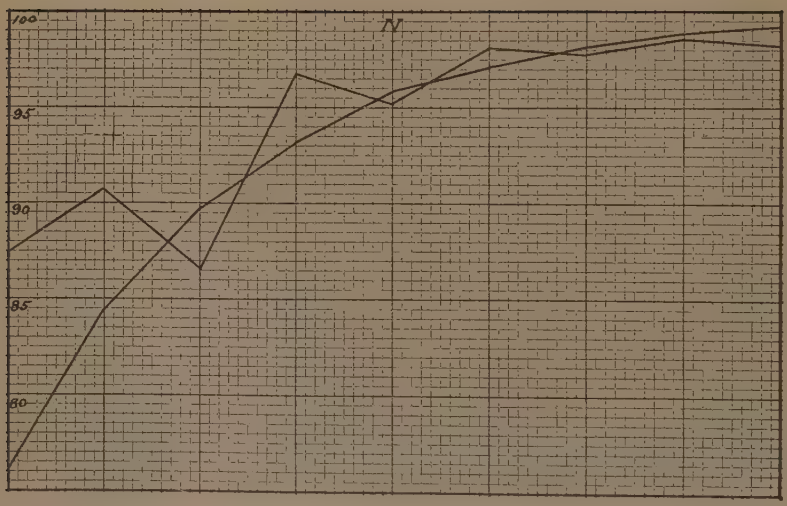
PLATE I

CONTROL CURVE, EXPERIMENTAL CURVE

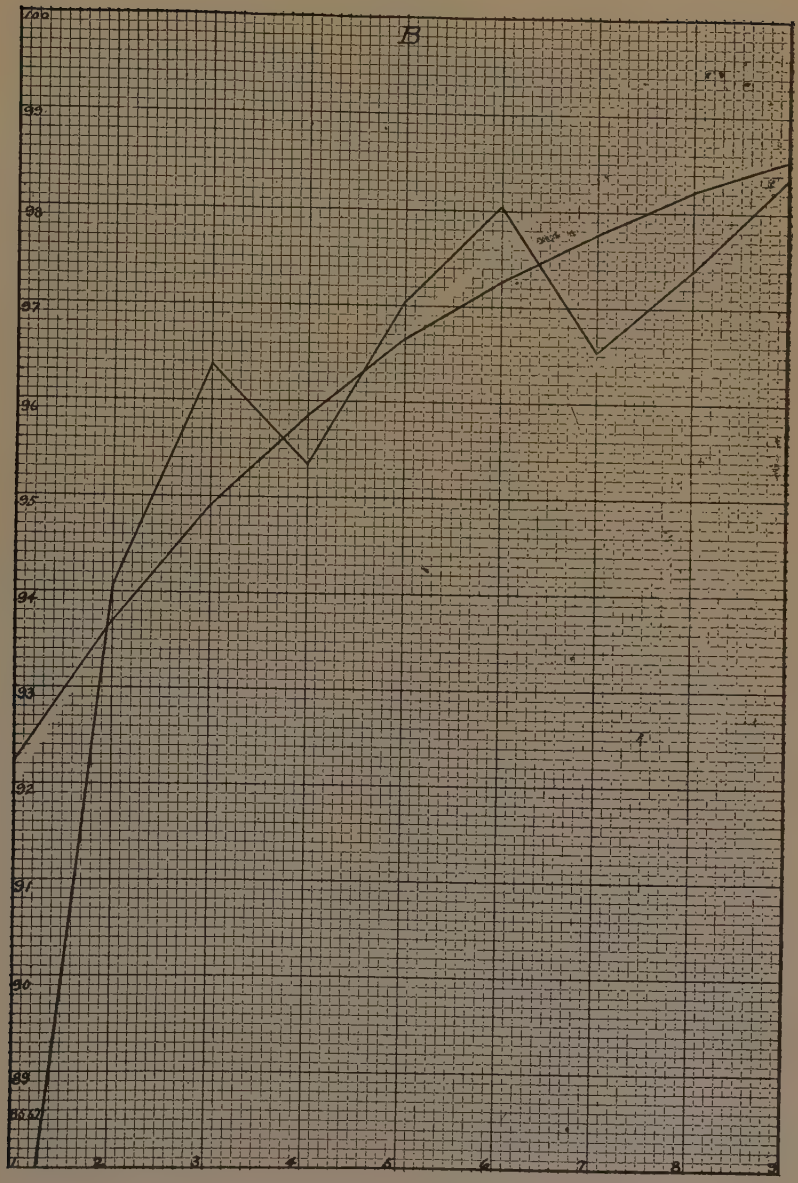
SUBJECT A

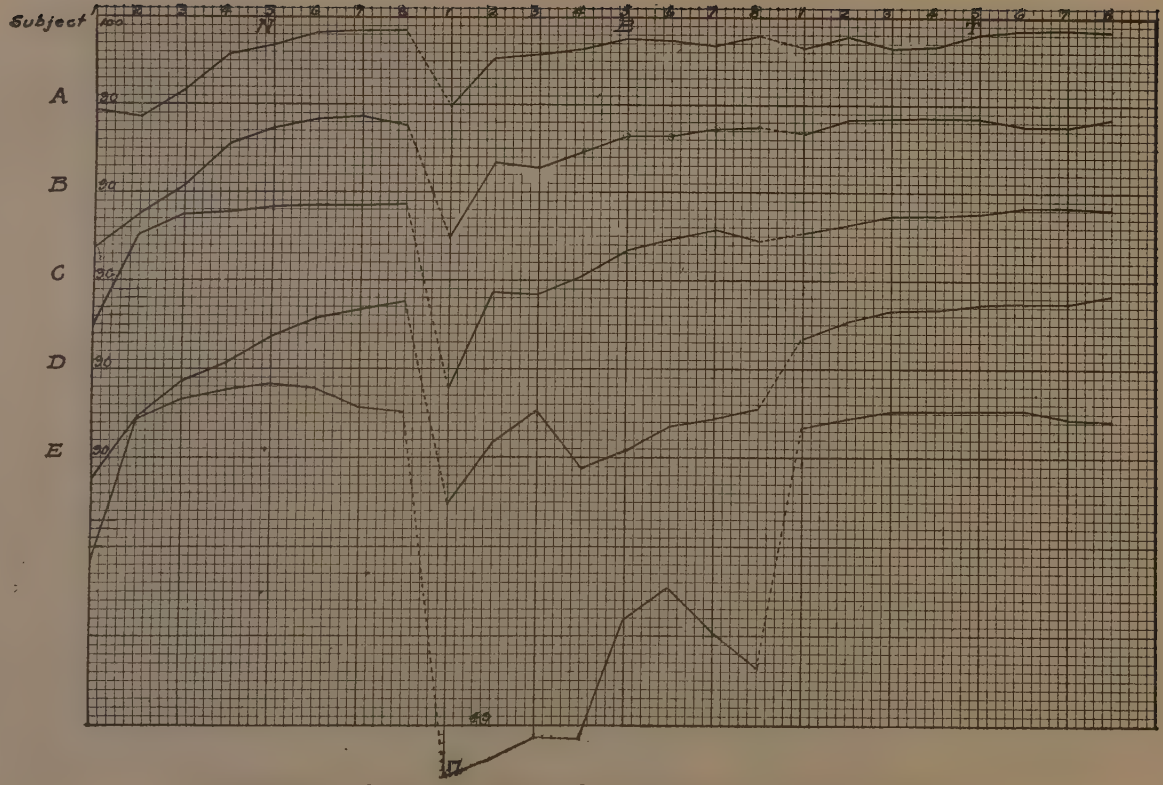


SUBJECT A



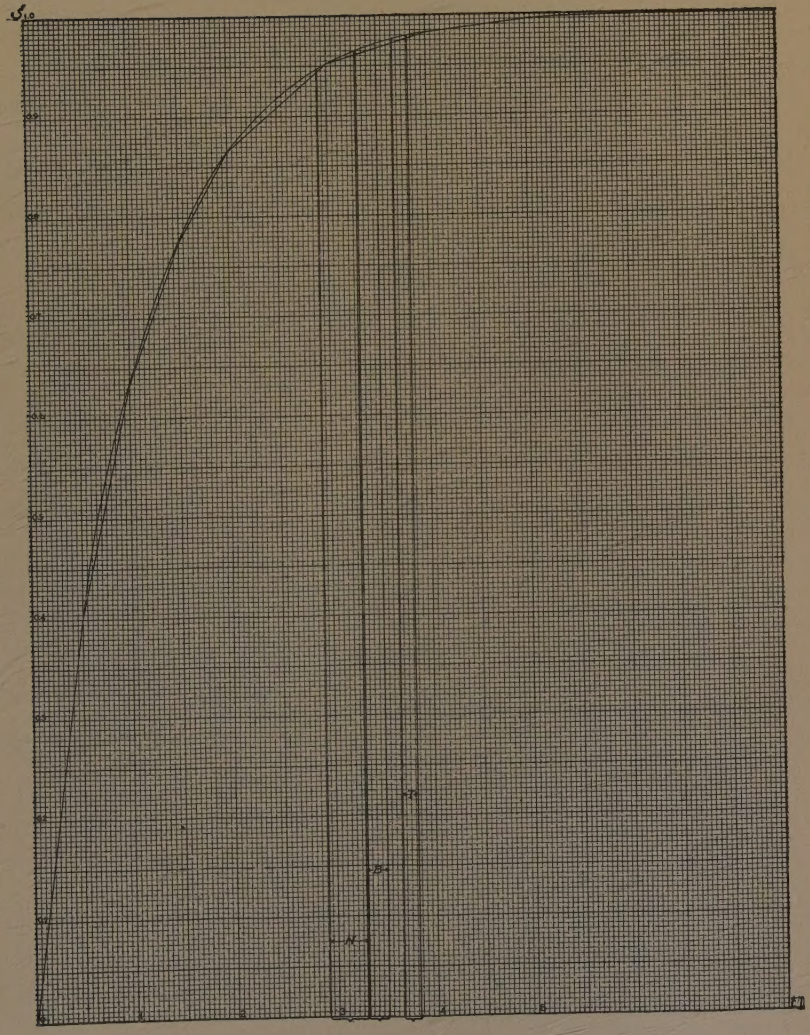
SUBJECT A





EXPERIMENTAL CURVES FLATTENED ONE DEGREE. (5 SUBJECTS, 3 TESTS)

PLATE III



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